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PLANETABLE METHODS AS ADAPTED TO GEOLOGIC MAPPING.

CARROLL H. WEGEMANN.

INTRODUCTION.

During the past six years the Fuel Section of the U. S. Geological Survey has been called upon to examine and classify, with reference to coal and oil, vast areas of the public domain in the regions of the Great Plains and Rocky Mountains. The mapping of so large an area in the detail necessary, and with the speed required, was a problem which demanded methods of work especially adapted to the conditions. Over much of the area the geologist was obliged not only to make a geologic map but also to construct a base map, either topographic or plain, on which to place the geology.

Several methods of rapid mapping have been tried with varying success in meeting the requirements, but the one which has proved itself best adapted to this kind of work is that of mapping with the planetable, whether by triangulation, stadia traverse, or a combination of the two. The following paper makes no pretensions of being an exhaustive treatise on planetable methods. Such a treatise should be written rather by the professional topographer than the geologist, but the writer hopes that a detailed description of planetable methods as adapted to geologic mapping in areas where no adequate base map is at hand may be

of general interest, following the paper by Mr. F. L. Ransome in a recent issue of this journal.

OUTFIT.

The outfit used on the plains is such as can be conveniently carried on horseback. It is as follows:

1. *Planetable.*—This is the usual 15-inch planetable and tripod fitted with the Bumstead head. The instrument is light and compact but is sufficiently steady to permit the use, except in very windy weather, of a light telescopic alidade. In horseback work, with a little practice, it can be carried on one's shoulder without removing the board from the tripod. A light cover made of waterproof canvas should be fitted over the top of the board to protect the map in case of rain.

2. *Alidade.*—This may be either an open sight or telescopic instrument according to the requirements of the work. A very compact telescopic instrument is that known as the Gale alidade, which was planned by H. S. Gale of the U. S. Geological Survey in 1909, and is unexcelled where a light compact instrument is required.

3. *Compass.*—A compass for orientation of the planetable is attached to the Gale alidade. If another style of alidade is used the ordinary box compass which can be obtained, if desired, with the planetable, is very satisfactory. This compass should not be attached to the board but carried separately and placed upon a line of declination laid off on the map when orientation is required. If a 10-inch open sight alidade is used the compass can be fastened, by any instrument maker, on the upper surface of the alidade ruler parallel to the edge, the uprights being adjusted so as to close at one side of the compass box. This arrangement obviates the necessity of carrying two separate instruments and at the same time makes the adjustment to the line of declination more accurate by increasing the length of the edge of the compass box.

4. *Paper.*—The paper usually employed on the Survey in field sketching on this sized board is the single mounted "Paragon" paper No. 116.

5. *Pencils.*—Good drawing pencils are necessary, the hardness depending on the preference of the user and the climate. In damp weather the drawing paper becomes soft and a 6 H pencil may be used, but in extremely dry weather a 7 H or 8 H is preferable. It is convenient to sharpen one end of the pencil in a long fine point for sketching and the reverse end in the form of a wedge for line drawing. A leather pencil case made to accommodate two or three pencils and a boxwood scale may be obtained of a size which will fit the vest pocket.

6. *Planetable Needles.*—Planetable needles may be made by mounting No. 12 Sharps in small rolls of sealing wax, allowing about three eighths of an inch of the needle point to project beyond the wax.

7. *Scale.*—A boxwood scale adapted to the scale of the proposed map is essential. It should show on one edge, miles divided into tenths, fiftieths, or hundredths as the case may be, and on the other side a corresponding scale representing feet.

8. *Protractor.*—A celluloid protractor of about 2½ inch radius should be carried for plotting strike readings.

9. *Hand Compass.*—A light and compact instrument which can be used not only as a compass but as a hand level and clinometer should be used.

10. *Barometer.*—A barometer is necessary unless elevations are determined by dip angles, and even then it is convenient in contour mapping if used advisedly. It is at best an unsatisfactory instrument, changing not only with every passing storm but usually with the diurnal change in temperature. If used it must be checked constantly on points of known elevation to avoid error.

A convenient means of carrying the above instruments, including the Gale alidade, is in a pair of canvas saddle pockets which are hung on the horn of the western saddle. When the geologist dismounts to make a "set-up," he can, if he wishes, slip the pockets from the horn of the saddle and carry them with him to the point where the planetable is to be set, thus having all his instruments at hand. The weight in the pockets should of course be equalized as much as possible to prevent injury to the horse.

SCALE OF THE MAP.

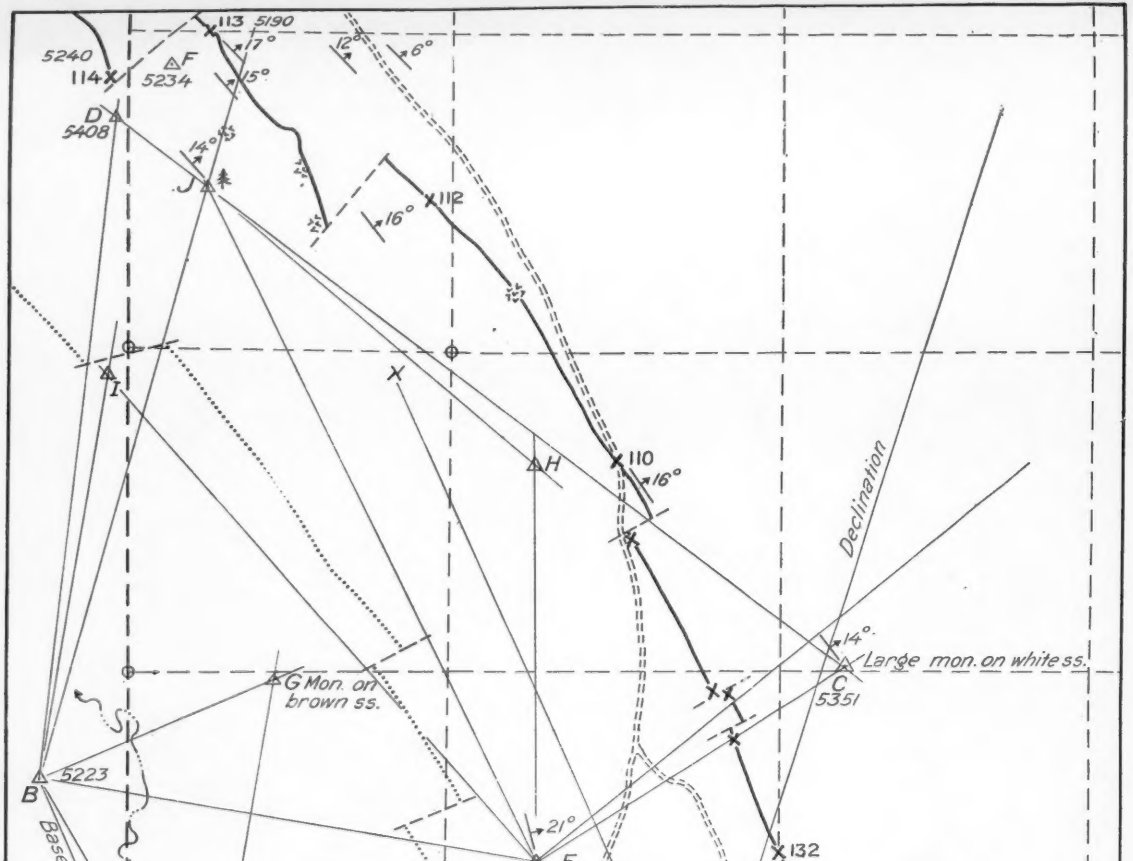
The scale of the map is determined by the amount of detail which is to be shown. The scale used in mapping the coal fields is generally two inches to one mile. By using this scale, a single township can be represented on one 15-inch planetable sheet leaving a margin of 1½ inches all around.

A scale of 4 inches equals 1 mile is much better adapted to very detailed work, but on this scale only a small area can be included on a single sheet, and it is necessary to establish a greater number of triangulation points for the area involved than would be necessary on a smaller scale.

A scale of 1 inch equals 1 mile permits of the assembling of all necessary geologic data where great detail is not required. Triangulation is established much more readily because of the fewer triangulation points necessary per square mile, especially in a region where the air is clear and signals can be seen with glasses at a distance of ten miles or more. Moreover, in any geologic study it is a great advantage to have the data for a considerable area assembled on one sheet while the work is being carried on.

LOCATION BY TRIANGULATION.

In fields where there is little or no timber, and prominent points can be seen for considerable distances, the method of location by triangulation is not only more accurate, but more rapid than that of location by stadia traverse. The accuracy of triangulation locations is dependent only on the scale of the map and is not subject to the errors which in stadia traverse are caused by inaccuracies in orientation of the planetable and by errors in plotting the distances of sights. In fields, however, where the instrument man is working in deep valleys from which he cannot sight distant signals, and where it is necessary to make numerous locations along coal beds or other important horizons, the method of stadia traverse is much superior to triangulation. Even in such a country, however, there is a great advantage in having a primary triangulation control from which certain points along stadia


113
5190
17°
12°
6°
5240
114
F
5234
15°
D
5408
14°
J
16°
112
I
X
H
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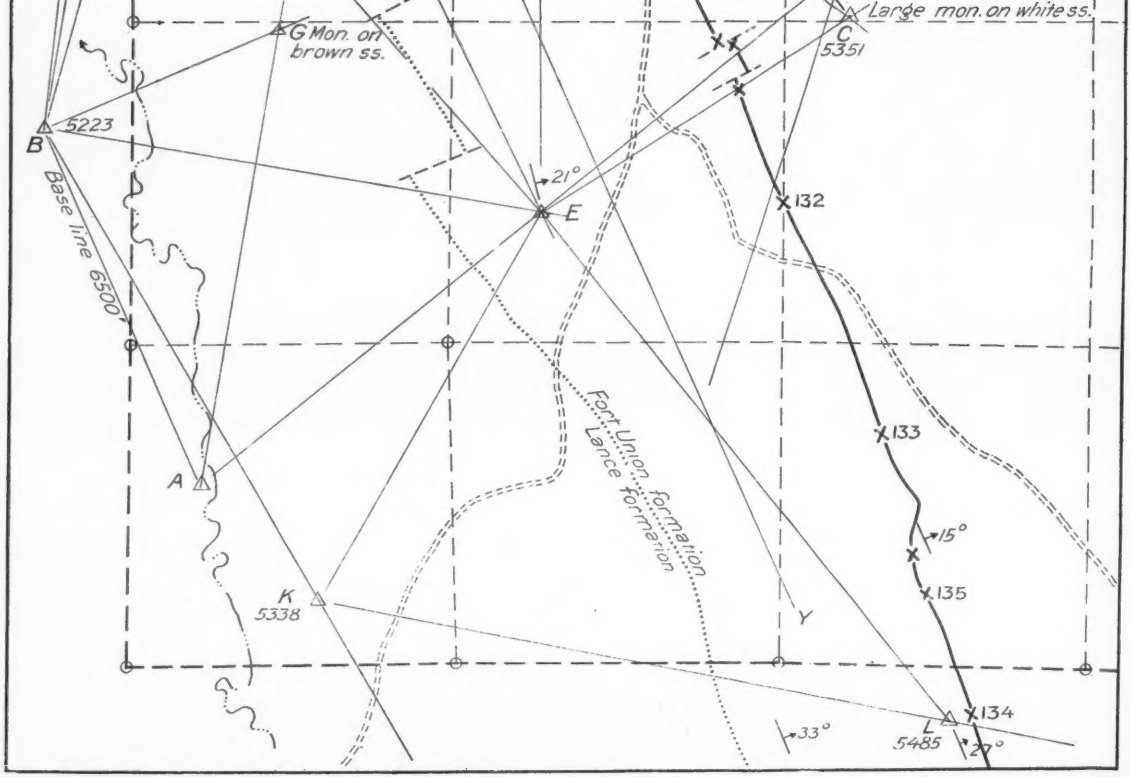
14°
Large mon. on white ss.
C
5351
G Mon. on brown ss.
5223
B
21°
132



Sketch of the Southwest part of a field sheet of T. 42 N., R. 77 W., Wyoming. Shows methods of triangulation and mapping in a planetable Geologic Survey.

traverses may be checked and adjustments in traverses made if necessary.

In triangulation work such as that done by the Fuel section, a primary control extending over the whole field is not necessary, though it would, of course, be most valuable where it could be easily obtained. A convenient method of work is to establish the primary control township by township, the net of triangles for the various townships being tied together by common points located along the margins of adjoining sheets.

METHOD OF PROCEDURE.

Plate V. is a sketch of the southwestern part of a field sheet of T. 42 N., R. 77 W., which was mapped by the writer. Let us suppose that we are about to construct this map. A strip of comparatively level ground along a stream valley is chosen on which to measure a base line, and the approximate length of the base line is determined by pacing. A monument of stone about 4 feet in height is built at its northern end which is on a slight elevation. The southern end on a broad flat near the stream is marked by a tripod of logs. The two signals are of course chosen so as to be intervisible and the exact length of the base line is chained in the ordinary manner. (If a steel tape and marking pins are not at hand, a 50-foot fiber tape and eleven large nails with a bit of white rag tied around each may be substituted.) The base line *AB*, shown on the plat, is 6,500 feet in length but is longer than is required on this scale of map. Generally speaking, in mapping on the scale of two inches equals one mile the base line should be about one mile in length.

The base line having been measured it is now necessary to decide where it is to be plotted on the planetable sheet. It is desired that the map of the whole township of 36 square miles shall fall on the sheet with an equal margin on all sides. A square 12 inches on a side is therefore laid off in soft pencil on the sheet to represent the boundaries of the township, the southwest corner of this square representing the southwest township corner. The location of this corner in the field is learned by inquiry from a neighboring rancher, it being but half a mile from the southern

end of the base line. Going to the corner, a reading with the hand compass is taken to the south end of the base line and laid off with the protractor on the planetable sheet. The distance is then paced on horseback (a horse paces as accurately as a man) and the distance scaled off on the map from the southwest township corner. The approximate location of the southern end of the base line is thus determined and a needle stuck in the sheet to mark it. This point (A) is also emphasized on the map by a small triangle. The planetable is now set up at point A, the southern end of the base line. The leveling of the board can be done by the eye, but is much more easily accomplished by the bubble provided on the Gale alidade for that purpose.

The magnetic declination used in running the land surveys in this area, and recorded on the township plats of the General Land Office, is about 17° east of north. The hand compass is set for this declination and by laying the compass on the planetable sheet parallel to its edge, and revolving the planetable board until the compass points north, the edge of the sheet is oriented approximately north and south. The board is now clamped in position and the alidade placed on the planetable with the ruler edge against the needle set at the point representing the south end of the base line, and the instrument sighted at the monument B marking the north end. With a hard pencil a fine line AB as long as space will permit is drawn in this direction. It will be seen that the alidade in this position is on one corner of the planetable board, rendering it somewhat unsteady. After the line is drawn the alidade is set over toward the middle of the board and again sighted at the north end of the base line, and the line xy drawn, the use for which will appear later. It is evident that the line xy is practically parallel to the line first drawn since in each case the alidade is sighted on the same point over a mile distant and the change of position of the instrument on the board is not sufficient at that distance to affect perceptibly the parallelism of the lines. The alidade is again set against the needle marking the south end of the base line on the map and lines drawn to the points K, E and G which have previously been marked by monuments or flags. It sometimes happens in taking sights from a

point near the edge of the sheet that the alidade is so near the edge of the board as to make it impracticable to use the ruler edge, although the back edge of the alidade could be used. There is no objection to this providing that the two edges of the alidade are exactly parallel. To test this, place the alidade on the planetable, sight on any distant object, and draw a line along the ruler edge. Now set the back of the alidade precisely on this line. If the vertical hair in the instrument still bisects the distant signal the two edges of the alidade ruler are parallel.

Returning to the case in hand, the alidade is next placed near the middle of the sheet and shifted until the compass on the alidade points exactly to magnetic north, and a line drawn along the ruler edge of the alidade indicating the declination. Whenever the alidade ruler is placed along this line and the board revolved until the compass points north the board will be correctly oriented provided that the compass is not affected by local attraction.

The planetable is next set up at *B*, the northern end of the base line, the ruler edge of the alidade in a reverved position is placed exactly on the line *xy*, and the planetable board revolved until the vertical hair in the alidade bisects the signal at *A*. The board is thus oriented as it was at the first set-up, since the line *xy* is parallel to the line *AB*. Had the alidade been placed directly on the base line *AB*, as is usually the case, the instrument would have been so near the edge of the board as to render it unsteady. Accurate orientation is very essential, for if the ruler edge of the alidade does not exactly coincide with the line on the paper an error will be introduced which may affect all the later work. It is often convenient to use a small hand lens in examining the coincidence of the line and ruler edge.

With the boxwood scale the distance of 6,500 feet is scaled off along the base line on the map, and the needle transferred to the new location *B*, representing the northern end of the base line. With the ruler edge of the alidade against the needle, sights are taken and lines drawn to *K*, *E* and *G*, and also to the new points *D*, *I* and *J*. The intersections of the lines drawn from the two ends of the base line at the points *K*, *E* and *G*

determine the locations of these points except in the case of the point *K* where the angle between the two lines is so acute as to make the accuracy of the location doubtful. It will be seen that if, in drawing either of the lines to *K*, the pencil is not held at such an angle that the line will be drawn from the center of the needle set at *B*, the point of intersection of the lines may be shifted a considerable amount, whereas if the angle between the lines were greater such an error would be scarcely appreciable.

The planetable is now removed to the point *J* and oriented by backsight on the line previously drawn to *J* from the north end of the base line. A needle is set at *E* already located by intersection from the two ends of the base, the alidade set against the needle and sighted on *E*, and the line *EJ* drawn, the intersection of this line with the line drawn to *J* from the north end of the base determining the location of *J*. The needle is next set at *J* and lines drawn to *D*, *H* and *C*. Other minor sights are taken at indications of coal or points of geologic interest the location of which may have value as the work progresses. The planetable is removed to *E* and oriented by backsight along the line drawn to *E* from the south end of the base line. The point *K* is now accurately located by the intersection of the line *E* with the two lines previously drawn. Sights are also taken and lines drawn to *L*, *C*, *H*, etc., and the work continued as before. It should be noted that in no instance while establishing the triangulation in the township is the planetable oriented by compass, but in every case by backsight.

A sufficient number of points having been located in the township to permit of the ready location of all secondary points necessary in the actual mapping, the next step is to find and locate some of the land corners. The instrument man therefore returns to the southwest township corner, the precise location of which has not yet been made. The planetable is set up over it and oriented by placing the alidade with a compass attached along the line of declination previously laid off and revolving the planetable board until the compass points to magnetic north. A needle is then set at the points *B*, *A* and *K*, in turn, the alidade sighted at these points, and lines drawn toward

the point required. The intersection of three lines in a point determines the location of the point required, or, in other words, of the section corner at which the set-up is made. Should the lines drawn from the three known points fail to intersect in an exact point, but form a triangle, it is apparent that the orientation of the planetable board is inaccurate. This is very often the case because of the presence near the instrument of substances affecting the compass, making an adjustment of the planetable necessary. It is possible to determine by inspection of the triangle the approximate location of the required point and thus greatly facilitate the orientation of the board. An excellent discussion of this subject is given in the Plane Table Manual by D. B. Wainwright, published by the U. S. Coast and Geodetic Survey as Appendix No. 7, Report for 1905, and the following brief statement is adapted from this work:

It is understood that the term "fixed points" refers to those already located on the sheet. The great triangle is formed by the three fixed points, and the great circle is the circle passing through them. The point sought will mean the true position on the sheet of the projected point of the station occupied.

"Rule 1. The point sought is always distant from each of the three lines drawn from the three fixed points in proportion to the distances of the corresponding actual points from the station occupied, and it will always be found on the corresponding side of each of the lines drawn from the fixed points."

That is to say, if the point falls on the right side of a line drawn from one of the fixed points as the operator faces the fixed point, it will fall on the right side of each of the lines drawn from the other two points when the operator faces them in turn.

"The simplest case for the application of this rule occurs when the station to be determined is within the triangle formed by the three fixed points; the point sought must then be within the triangle of error to satisfy the conditions."

"Rule 2. When the point sought is without the great circle, it is always on the same side of the line from the most distant point as (is) the intersection of the other two lines."

"Rule 3. When the point sought falls within either of the three segments of the great circle formed by the sides of the great triangle, the

line drawn from the middle point lies between the point sought and the intersection of the other two lines."

"When the new station lies on or near the great circle its position is indeterminate."

"In practice the topographer first decides the relation of the new station with reference to the fixed points, whether it is within the great triangle or in one of the segments, or outside of the great circle. He then determines the position of the point sought with reference to one line (if within one of the segments or without the great circle, by rule 2 or 3); it then follows from rule 1 that it must be on the corresponding side of the other two lines. Finally, he estimates the relative distances of the three actual points from him, and marks the position of the point sought a proportionate distance from the three lines."

The alidade is next placed in coincidence with the point sought and the location of one of the three signals just used in drawing the triangle of error and the board turned sufficiently to bring the instrument to bear on the signal. The triangle of error being erased a new set of lines is drawn from the three fixed points. If these lines meet in an exact point the orientation is correct, if a new triangle is formed the location of the point sought was not correctly estimated from the triangle of error and the operation must be repeated.

A convenient method of orientation when a sheet of tracing paper or transparent celluloid is at hand is described by Mr. Ransome in his article already referred to in the February–March number of this journal.

Turning again to the mapping of the area under discussion, it is evident that after one land corner in the township has been found and located, it may be used as a starting point in the search for other corners. A sight is accordingly taken with the compass in the direction of the next section corner (in the present case either north or east) and the instrument man rides in the direction chosen, keeping count of the paces of his horse. A convenient means of keeping this count is a tally register which, carried in the hand, is operated by pressing a small lever. The instrument registers from one to one thousand. The operation of it becomes almost automatic, the lever being pressed by the thumb as the horse puts down his right front foot. The average horse takes about 950 double paces in a mile, but there is con-

siderable variation in individuals, and every horse should be tried out on a half-mile on fairly level ground. After becoming accustomed to a particular horse and learning to allow for his variation in pacing up and down hill, a man can estimate distances very accurately, a mile within five paces for example.

Several corners along the township line having been found, and located in a manner similar to that used in locating the township corner, a provisional land net is constructed from them as an aid in finding the remaining corners. After this is done, when the instrument man sees from the map that he is near the probable position of a corner, he can, by setting up his instrument, determine his exact position with reference to the corner without the trouble of riding the section lines.

The primary control having been established, and the provisional land net drawn, we are ready to begin the geologic mapping and will turn our attention first to the coal outcrop which crosses the area in a general northwest-southeast direction. The work up to this time has probably consumed from one and one-half to two and one-half days, and while it has been in progress one of the members of the field party has been engaged in opening up the coal bed at the more advantageous localities. He has taken measurements of the bed and recorded them in his notebook by numbers, leaving a slip of paper with a corresponding number at each locality where a measurement was made. In mapping the coal bed, therefore, it becomes necessary to record these numbers on the map.

On visiting localities 113 and 114 at the northwest corner of the area it is found that both lie in a deep valley from the bottom of which it is possible to sight only the signal *J*. Lines have already been drawn to these exposures from *J* when that station was occupied in establishing the primary control; the planetable is therefore set up at *F*, a high point from which both 113 and 114 are visible as well as several of the triangulation stations in the township. *F* is located from the signals *D*, *J* and *C* by precisely the same method as that used in locating the southwest township corner. It is well to make a distinction on the map between the primary control points and secondary lo-

cations made by the three point method. An inverted triangle is therefore used to mark station *F*. From *F* sights are taken and lines drawn to coal openings 113 and 114, and these places are thus located by the intersection of these lines with the lines previously drawn to them from *J*. The elevation of *F* is determined by dip angles (see p. 635), and likewise the elevations of the two coal openings are determined by observations from *F*. After the two coal openings are located, the fault which breaks the coal outcrop between them can be readily drawn in. When station *J* was occupied, sights were taken to the three red hills near the middle of the section. These hills are capped by the baked rock formed by burning coal. They are now cut in from point *F*, and it is easy to sketch in the coal outcrop as far as the fault which breaks it in the SE. ¼ of the section. On leaving point *F* the geologist takes the two dip readings along the coal outcrop and records their approximate location on the map. He next rides along the coal crop to the fault, recording in his notebook what observations are necessary, and sets up his instrument at the rock outcrop on which a dip reading of 16° is taken. This outcrop is located by the three point method as above described, and a sight is taken at the place where the fault breaks the coal outcrop directly north of this location; also at the red hill in the SW. ¼ of the section to the east. Coal opening 112 is now visited and the instrument set up at it. A sight is taken to the northwest along the outcrop of the coal and the location of the northern end of the fault is thus made, the fault being sketched in from this intersection and that of one of the red hills previously located. A sight is also taken to the red hill in the SE. ¼ of the section to the east, and the coal outcrop sketched as far as that hill. Location 110 is made by the three point method, the instrument being set up at the coal outcrop. Triangulation station *C* is next occupied and various sights taken from it, and thus the work is carried on from point to point across the rest of the area. As the geologist rides in to camp from his work he makes occasional set-ups along one of the roads and is thus able to map a part of it.

The outcrop of the stratum marking the boundary between

the Lance and Fort Union is located either by setting up the instrument directly on it or by cutting it in from known points, and the boundary is sketched by the eye from these various locations. Occasionally when the geologist finds himself near a section corner he stops to find it and to locate it by the three-point method; or he pauses, when he has opportunity, to sketch the roads and the larger streams.

It is often convenient when two or more men are working from the same camp to transfer the primary control from the first sheet to a second. This is done by laying the original sheet over the second sheet and pricking through the control points with a fine needle on to the second sheet. One man can then map geologic boundaries, for example, while the other is mapping the coal, and the information recorded on the two sheets can be later compiled in the office on one sheet.

GENERAL SUGGESTIONS.

Both in the geologic mapping and topographic sketching the rapidity of the work depends much upon the ability of the operator to choose the most advantageous points from which to do his sketching. There is much also in the selection of the points which are to be cut in from the principal stations. For example, in drawing a small stream valley, sights can be taken to the junction points of side streams with the principal stream. In case the stream is of considerable size and is partly hidden by trees, sights may be drawn along the outside of the various bends in the stream which are visible, it being the operator's endeavor always to sight at objects which he can recognize from the next station which he intends to occupy. Preliminary contour sketches should always be made of topographic features on which sights are taken. After a few principal points are located in a township the approximate location of intervening points may be easily estimated, so that the preliminary sketches of these points may be made on the map in approximate relation to each other. This is a considerable aid in recognizing the same points from future stations. Time is never wasted by making careful preliminary sketches or in spending a few moments in taking a

last view of a landscape from a station on some high hill before leaving it to make sure that no important points have been omitted. Better too many sights than too few. If the preliminary sketches are well made the operator need not hasten to "cut in the points" from a second hilltop lest he forget at what points the sights were taken. The hills will be recognized from the sketches at any future time. This is an important factor in the work since the geologist must be at liberty to pause in his mapping at any time in order to give his attention to purely geologic problems.

It is sometimes convenient for the several members of a field party to work over the same field sheet on successive days. As each man usually prefers to use the alidade to which he is accustomed the change of field sheets involves the use of several alidades on the same map. The line of sight on an alidade is not necessarily parallel to the ruler edge, and the accuracy of the work is not impaired by this lack of parallelism providing that the same instrument is always used with a particular orientation. If another instrument is used which does not correspond to the instrument with which the map was begun a new declination must be laid off for the second instrument, or every location checked carefully by the three point method.

In beginning control by triangulation on a 15 inch planetable sheet using a scale of 1 inch equals 1 mile, a base line only 1 mile in length is too short to be used without danger of error. Control may be begun from a base line of this length on a scale of 2 inches equals 1 mile and two prominent hilltops several miles apart located in the usual manner. The distance between these points being divided in half and the points transferred to a new sheet they serve as a base line from which control may be expanded on the scale of 1 inch equals 1 mile.

In establishing control over a township adjacent to one already mapped it is not necessary to measure a new base line providing that no errors appear in the work. Two points located near the margin of the old sheet may be transferred to the opposite margin of the new sheet and used as the ends of a base line from which to expand control over the new area.

CARRYING ELEVATIONS BY MEANS OF DIP ANGLES.

The telescopic alidade, in combination with the planetable, may be used as a level in carrying elevations. In ordinary geologic work, however, the difference in elevation between two known points is calculated, with the aid of tables, from vertical angles taken with the telescopic alidade from one point to the other. The horizontal distance between points located on the planetable sheet can be easily measured with a scale marked in units of a hundred feet. A correction for curvature of the earth must usually be added or subtracted, when the distance between the points is from 1 to 6 miles. Angular readings and all computations should always be recorded in the notebook to which reference can be made later in case of error. When elevations are carried over a considerable area in the above manner, it is apparent that if one mistake is made in the elevation of a particular point this error will affect the elevation of all succeeding points dependent on it. It is therefore advisable to determine the elevation of two or more points approximately on a line, for example, along the north border of a township. The elevation of each of three points along the south border of the township can then be determined by taking readings upon each, from each of the points already determined along the north of the township. In this manner a constant check may be kept on the accuracy of the work as the elevations should agree with each other within 5 feet or at most within 10 feet, and by taking the average of several elevations calculated for one point, a closer approximation to the true elevation is obtained. It is of course apparent that elevations carried in this manner are by no means absolute, but they are, as a rule, sufficiently accurate for most purposes of geologic mapping.

The location on a planetable sheet of various points along geologic boundaries, and the determination of the elevation of these points, together with the recorded strike and dip of the strata, give the geologist material from which to construct geologic sections. The advantage of the planetable method over methods of hand leveling is that the complete record of all the work appears on the map and may be checked at any time in

the office. It is possible also to compile directly from the map at any time stratigraphic and structural sections the need for which was not appreciated in the field.

STADIA TRAVERSE.

The advantage of using triangulation methods in combination with the ordinary stadia traverse is apparent. When locations are made only by stadia traverse it is necessary to continue the traverse across unimportant areas, making numerous instrumental locations which have no bearing on geologic problems and are made purely to "carry the line." By the use of triangulation the most advantageous points may be selected at any time from which to begin stadia traverses, and the stadia traverses may be checked at intervals by triangulation locations in order to correct the errors which necessarily creep into traverse lines due to inaccuracies in scaling distances and errors in orientation. In doing stadia work the geologist cannot work alone but must have a rodman. Stadia traverse, either in connection with triangulation or used independently, is particularly adapted to work in which it is necessary to map in great detail the outcrop of one particular stratum, such as a coal bed. It is also most valuable in detailed geologic work along deep valleys or through timber.

A long traverse line may be run or the instrumental locations may be made entirely by triangulation, the rod being used to locate numerous minor points about the principal stations. The rod adopted by the Fuel Section for stadia work is jointed at the middle and is 13 feet in length. It thus permits of sights up to 1300 feet in length, or 2600 feet in case the middle cross-hair of the instrument is used. Usually in "carrying a line," the rodman waits while the instrument man makes his observations for distance and altitude, records them, and then rides past him to the next station. When the backsight is completed from this station the instrument man waits while the rodman passes him and rides forward to his new station when the observations of the foresight are again made. Under some conditions it is more advantageous for the rodman to mark his station and

for both the rodman and instrument man to ride forward at the same time, the instrument man occupying the station just left by the rodman and the rodman taking a new station. Although this method saves time in one way, it requires double the number of "set-ups" of the instrument and makes the orientation by backsight impossible unless the instrument man can mark his last station so that he can recognize it at a distance. Compass orientation can be used satisfactorily unless there be local attraction such as is frequently encountered when in proximity to the baked rock formed by the burning of coal beds, to magnetic sands, to oil pools, etc.

TIME REQUIRED FOR THE WORK.

The time required to measure a base line and establish primary control over a township varies under ordinary conditions from one to three days. The instrument man is assisted in measuring the base line and usually in setting up signals at the various triangulation stations. Aside from this he works alone except when stadia work is done.

The time required to set up and orient the planetable by the three point method is generally from five to ten minutes, the amount of time spent at each station after the instrument is in position depending entirely on the number of observations to be made.

In running stadia traverse the planetable can be set up and oriented and the readings for distance and elevation to one position of the rod made in five or six minutes. The time necessary, after the base line is measured, for one man to examine and map an area such as that shown on the accompanying sketch may be estimated at from four to six days. It is obvious that the amount of time is dependent largely on the nature of the geologic problems involved.

THE PARAGENESIS OF MINERALS.

AUSTIN F. ROGERS.

INTRODUCTION.

The most interesting field in mineralogy at present, as well as the most promising one for the future, is probably that dealing with the occurrence, association, and origin of minerals. The mineralogist has too often been content with the measurement of crystals and the chemical analysis of a mineral, and has usually neglected to note the mode of occurrence and possible origin, except as they may be observed in the hand specimen. But as Lacroix[1] says: "Lorsque les propriétés physiques et chimiques d'un minéral sont definitivement établies, son histoire n'est pas complète. Il joue un certain rôle dans la nature; . . ."

Questions of occurrence and origin will more and more concern the mineralogist for the characters of minerals have been pretty thoroughly worked out, and in the future the occurrence, association, and origin will take equal rank with the characters themselves.

USE OF THE TERM PARAGENESIS.

In the discussion of the occurrence, association, and origin of minerals, the term *paragenesis* is often used and the fact that it is used in different senses by different writers is the principal reason for presenting this paper. The word *paragenesis* was coined by Breithaupt in 1849 and used as the title of his book, "Die Paragenesis der Mineralien."[2]

In the introduction (p. 1) of this work, Breithaupt defines *paragenesis:* "Unter der Paragenesis der Mineralien ist die mehr oder weniger ausgesprochene Weise des Zusammenvorkommens

[1] "Mineralogie de la France et de ses Colonies," Vol. I., p. vii, 1893.
[2] Pub. by J. G. Engelhart, Freiberg, 1849.

der Associazion derselben zu verstehen. Man hat dabei auf das relative Alter der Körper, da, wo eine Sukzession derselben zu erkennen ist, einen besondern Werth zu legen, weil in diesem Verhalten die meiste Belehrung liegt." Breithaupt emphasizes the order of succession, but his book is not confined to this topic. He discusses the occurrence and association of minerals in rocks and veins.

The following definitions of *paragenesis* are given:

Standard Dictionary: ". . . treats of the association and order of succession of mineral species in their formation."

International Dictionary: "The science which treats of minerals with special reference to their origin."

Century Dictionary: "The association of mineral species with each other with reference to the order and mode of their formation."

Rinne: "Das Zusammenvorkommen der Mineralien nennt man ihre Paragenesis."

Loewinson-Lessing: "Nennt Breithaupt die Gesetze der Association der Minerale in Gesteinen, Gängen, etc."

Kemp: "A general term for the order of formation of associated minerals in time succession one after another. To study the paragenesis is to trace out in a rock or vein the succession in which the minerals have developed."

Moses and Parsons: "The term *paragenesis* is used to express the relations between associated minerals."

A casual search of recent geological literature also shows great diversity in the use of the term *paragenesis.* I quote several illustrations:

Simpson:[1] "Recent studies in the order of formation or paragenesis of the metallic ore minerals . . ."

Palache,[2] speaking of powellite in Michigan, says: "It differed from the Idaho mineral in its paragenesis, occurring with native copper and epidote. . . ."

Clarke,[3] speaking of zeolites, says: ". . . but they represent

[1] Econ. Geol., Vol. 3, p. 628, 1908.
[2] *Amer. Jour. Sci.* (4), Vol. 7, p. 367, 1899.
[3] Bull. 330, U. S. Geol. Surv., p. 350, 1908.

some paragenetic relations. Stilbite and heulandite seem, for example, to be derivatives of an unknown calcium-albite."

Knopf and Schaller:[1] "It is clear from the paragenesis of paigeite that it represents a fixation product of magmatic emissions rich in fluorine (and chlorine), boron and iron."

Rogers:[2] "In characters and paragenesis, the Kansas anhydrite resembles the foreign anhydrite."

Thus we have *paragenesis* used in the sense of (1) order of succession, (2) association, (3) origin, (4) association and ocurrence giving a clue to the origin, and (5) a general term for the occurrence, association, and origin.

In the literature, the term *paragenesis* has perhaps been used more often in the sense of order of succession than in any other one sense. Breithaupt put great emphasis upon this phase of the association and probably thought that he had discovered important laws governing the order of succession, but we know that these are largely of only local importance. Yet many writers have put so much emphasis upon the order of succession that they use *paragenesis* as a synonym of order of succession. I protest against its use in this sense, for it is not only a limitation of Breithaupt's original meaning but substitutes the word *paragenesis* for the simpler and more definite phrase, *order of succession.*

I would define *paragenesis* as *the association of minerals with special reference to the occurrence and origin.* While it is not synonymous with *origin,* the idea of origin is involved etymologically. As Tschermak[3] says: "Das Zusammenvorkommen drückt also zugleich das Nebeneinander- oder Nacheinanderstehen, zuweilen auch die Abstammung aus, daher die von Breithaupt eingeführte Bezeichnung Paragenesis glücklich gewählt erscheint."

The ideas of occurrence, association, order of succession, and origin are so bound together that I should like to see the term *paragenesis* expanded so as to cover all these ideas. We have the

[1] *Amer. Jour. Sci.* (4), Vol. 25, p. 325, 1908.
[2] *Amer. Jour. Sci.* (4), Vol. 29, p. 258, 1910.
[3] "Lehrbuch der Mineralogie," 6th edit., p. 318, 1905.

separate terms enumerated but no single term except *paragenesis* which can be used in the sense indicated. At any rate, *paragenesis* means the relations between associated minerals, and not simply one phase of these relations.

PARAGENETIC VARIETIES.

The present tendency in mineralogy is to do away with varietal names, based upon trivial characters, and to emphasize the unity of a mineral or mineral species, based upon essential characters. The varietal names based upon crystal habit, structure, color, and slight differences in composition are gradually disappearing from mineralogical literature.

The present tendency is to base varieties upon occurrence, association, or origin, or, in short, upon paragenesis; hence, the term *paragenetic varieties* used in a recent text-book.[1]

This term is perhaps better than genetic varieties, for the origin of a mineral can not always be determined, and it may also be desirable to distinguish several kinds of occurrence with the same origin; for example, a mineral found in two different kinds of igneous rocks with different associates, yet in each case a product of fusion.

Tschermak[2] proposes that all specimens with the same essential properties be called a genus (*Gattung*) and that all specimens of any genus with the same manner of formation (*gleiche Bildungsweise*) be called a species (*Art*). According to Tschermak, the manner of formation impresses the mineral with a characteristic form or texture. While there is often a correlation between the form or texture, and the kind of occurrence, yet I believe Tschermak attaches too much importance to it. As Miers[3] says:

"The essential characters of a mineral, moreover, are quite independent of its source or previous history. . . ."

Tschermak establishes species (*Arten*) for several minerals (*Gattungen*). For example, there are five for magnetite, as follows:

[1] Rogers, "Introduction to the Study of Minerals," New York, 1912.
[2] "Lehrbuch der Mineralogie," 6th edit., p. 370, 1905.
[3] "Mineralogy," p. v, 1902.

1. Attached (*sitzende*) crystals (in the clefts of crystalline schists).
2. Embedded (*schwebende*) crystals (in crystalline schists).
3. Small octahedra and grains as a constituent of igneous rocks.
4. Granular magnetite interbedded with schists.
5. Secondary deposits in the form of sand.

Tschermak establishes definite species (*Arten*) for only about three minerals. For calcite and dolomite he says that the species fall under three divisions (*Abteilungen*), crystallized, crystalline, and limestone; but he does not definitely state what the species are.

Klockmann,[1] in his "Lehrbuch der Mineralogie," gives a numbered list of paragenetic varieties for a large number of minerals, though he does not use the term *paragenetic*. This, I believe, marks a distinct advance in the science of mineralogy. Magnetite he lists under four kinds of occurrences:

1. As an original constituent of basic igneous rocks.
2. As layers and lenses in regionally metamorphic rocks.
3. As contact deposits.
4. As sand.

From the standpoint of origin, this is a better list than that of Tschermak, though it might be improved by several additions.

Lacroix, in his "Mineralogie de la France et de ses Colonies," which may well serve as a model for all future work in regional mineralogy, gives a detailed description of minerals not by localities primarily, but by geological mode of occurrence, numbering the kinds of occurrences. He considers magnetite, for example, under seven heads.

1. In eruptive rocks.
2. In the contacts of eruptive rocks.
3. In the metalliferous veins.
4. In the seams and clefts of various rocks.
5. In the crystalline schists.
6. In the sedimentary rocks.
7. In artificial products.

[1] 3d edition, 1903; 4th edition, 1907.

There are subdivisions under some of these. Although Lacroix does not use the terms paragenesis and paragenetic varieties, the varieties he establishes are based upon paragenesis rather than upon origin alone.

For the first time in an English book,[1] the writer has attempted to give the most important paragenetic varieties for the two hundred minerals treated, although doubtless there are mistakes of both omission and commission. To establish, or to attempt to establish, these varieties may seem an ambitious undertaking. But science is an orderly arrangement of facts, and there should be an attempt to make definite our knowledge of this interesting part of mineralogy, which is, perhaps, in the same condition that the classification of minerals was half a century ago. To be convinced of this fact, let one attempt to get an adequate conception of the occurrence and association of some of the common minerals from our mineralogical text-books and treatises.

Certain occurrences and associations are so characteristic that they should be emphasized by numbered varieties. The time has come for definiteness of statement of these things even at the sacrifice of accuracy and completeness.

TYPES OF MINERAL DEPOSITS OR OCCURRENCES.

The discussion[2] of the paragenesis of minerals involves a classification of mineral occurrences, including not only rocks but veins and other mineral deposits.

W. H. Emmons,[3] in a paper entitled "A Genetic Classification of Minerals," amplifying an earlier paper of Lindgren's,[4] lists eight kinds of occurrences as follows:

1. Igneous Rocks.

[1] "Introduction to the Study of Minerals," New York, 1912.

[2] There are chapters on this topic in the German text-books of Klockmann, von Kobell, Naumann-Zirkel and Tschermak, and in the English text-books of Moses and Parsons, and Rogers.

[3] ECON. GEOL., Vol. 3, p. 611, 1908.

[4] "The Relation of Ore-Deposition to Physical Conditions," ECON GEOL., Vol. 2, p. 105, 1907.

2. Pegmatite Veins.
3. Contact Metamorphic Deposits.
4. Deep Vein Zones.
5. Deposits of Moderate and Shallow Depths. Igneous Rocks nearby.
6. Deposits of Moderate and Shallow Depths. No Igneous Rocks nearby.
7. Zone of Oxide and Sulphide Enrichment.
8. Products of Dynamo-Regional Metamorphism.

To make this list complete, I would add a number of others, for it has a broader aspect than the study of ore-deposits, important though that may be.

1. Igneous Rocks.
2. Meteorites.
3. Volcanic Sublimates.
4. Inclusions in Volcanic Rocks.
5. Pegmatites.
6. Pyroclastic Rocks
7. Sedimentary Rocks.
8. Sands and Gravels.
9. Organic Deposits.
10. Deep Veins.
11. Shallow Veins.
12. Oxidized Zone of Veins.
13. Zone of Secondary Sulphide Enrichment.
14. Saline Residues.
15. Contact Metamorphic Zones.
16. Regional Metamorphic Rocks.
17. Minerals of Recent Formation.

A UNIVERSITY COURSE IN THE PARAGENESIS OF MINERALS.

A university course in which minerals are studied from the standpoint of paragenesis is perhaps the most useful advanced course in mineralogy for most students. In an elementary course in mineralogy very little attention can be given to this subject, for it takes all of the student's time to master the char-

acters of minerals, especially if their sight identification is emphasized, as I believe it should be. As Miers[1] says:

"One important branch of scientific mineralogy is the study of the modes and places of occurrences of minerals, their origin, and the changes to which they are subject. This is to a large extent an entirely independent subject except in so far as it throws light upon the nature of minerals themselves; it is, moreover, a study which should succeed that of the essential properties of minerals, for the essential properties do not vary with the origin or the modes of occurrence."

A course of the kind should also be taken after elementary petrography, for it correlates the facts of mineralogy and petrography. It trains the student in observation, makes him familiar with important literature, and above all, makes definite his ideas concerning the association, occurrence, and origin of minerals. This course serves as an excellent introduction to the study of ore-deposits and may well be made a prerequisite to the economic geology course.

Such a course has been given by the writer at Stanford University for several years, though it is an optional course for advanced students and not a prerequisite for economic geology. A collection to illustrate the course is being gradually made. In this collection are assembled pseudomorphs, petrifactions, alterations, artificial minerals, minerals of recent formation, and all available specimens illustrating any phase of the occurrence, association, or origin of minerals. There is one very instructive specimen from Cornwall, England. This specimen consists of galena and chalcopyrite in vein quartz, while the free surface consists of limonite with acicular crystals of cerussite. Here we have on a small scale a typical ore-deposit with gossan above and unaltered sulphides below. Such specimens cannot fail to interest and impress the student.

SUMMARY.

When the physical and chemical properties of a mineral are worked out, there yet remains the task of learning everything possible about its occurrence, association, and origin.

[1] "Mineralogy," p. 5, 1902.

The term *paragenesis* has been used with various meanings, more often perhaps in the sense of *order of succession. Paragenesis* may be defined as *the association of minerals with special reference to their occurrence and origin,* but it seems advisable to expand the term so as to include the closely related ideas of occurrence, association, order of succession, and origin.

It is important to distinguish definite varieties of minerals based upon paragenesis, rather than upon origin alone. These may be called *paragenetic varieties.*

Seventeen types of mineral occurrences are enumerated. This is an expansion of the earlier list of Emmons.

A university course in the paragenesis of minerals is suggested as a useful advanced course. To be effective, it should be illustrated by a special collection of minerals.

THE LACK OF ASSOCIATION OF THE IRREGULARITIES OF THE LINES OF MAGNETIC DECLINATION AND THE PETROLEUM FIELDS.

W. A. Tarr.

The variation in the magnetic lines of declination has been advanced by Dr. Becker as evidence compatible with the theory that petroleum deposits are generated from iron carbides and in his paper he includes a chart of the isogonics with the oil fields plotted upon it.[1] This paper states further "what the map does prove is that petroleum is intimately associated with magnetic disturbances similar to those arising from the neighborhood of minerals possessing sensible magnetic attraction, *i. e.*, iron, nickel, cobalt, or magnetite. Henceforth, no geological theory of petroleum will be acceptable which does not explain this association."[2]

The writer of this paper does not agree with Dr. Becker that the map shown proves that there is an intimate association between the sinuosities of the lines of declination and the petroleum fields. There are even greater variations in other areas of the United States, *i. e.*, in Iowa, Nebraska, and Oregon, and if one agrees with Dr. Becker, one must conclude that oil should be found in these areas if the structural conditions for accumulation are present, or that there is no connection between the variations and the origin of the oil. If one studies the chart of 1910, the non-coincidences are just as striking as the coincidences. (See Fig. 75, which shows the lines of equal magnetic declination for the periods 1905 and 1910.)

THE GENERALIZATION IS BASED ON INSUFFICIENT DATA.

The magnetic charts, as at present constructed, are too generalized to form the basis for such an assumption. One needs only

[1] Becker, G. F., Bulletin 401, U. S. Geol. Survey.
[2] *Ibid.*, p. 24.

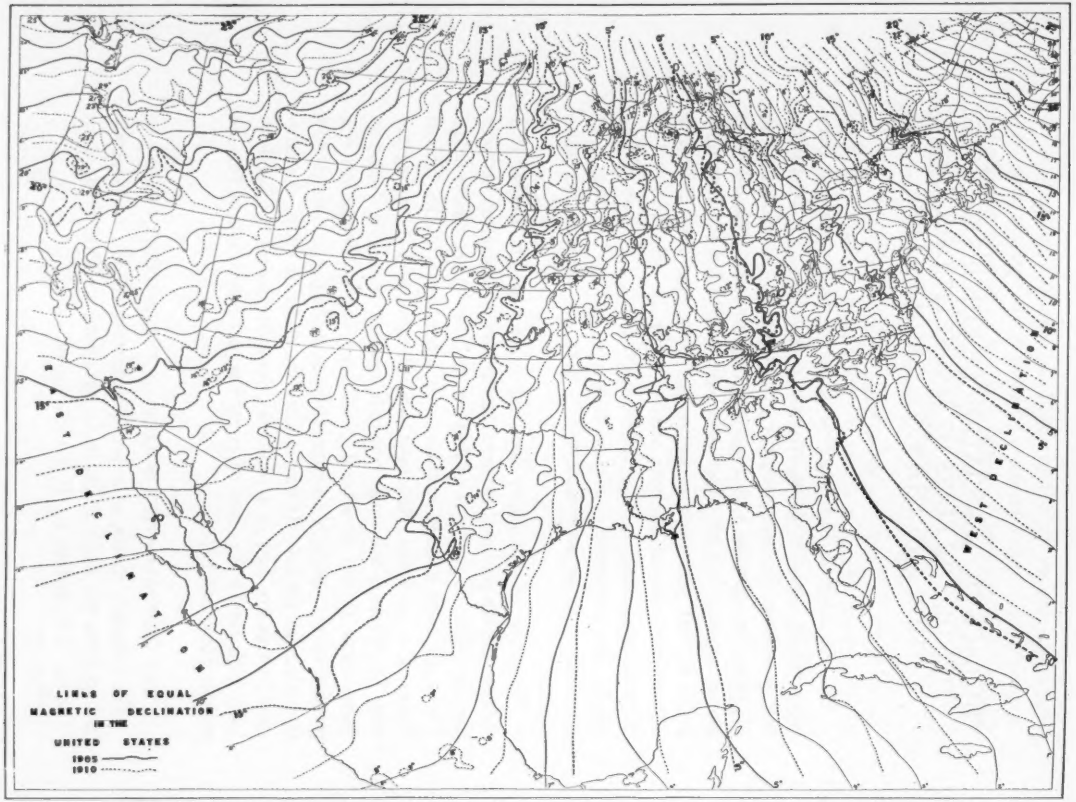


FIG. 75. Map showing the lines of equal magnetic declination for 1905 and 1910 and how the lines have changed in five years. Note the greater irregularities for 1910 than for 1905. (After charts of Coast and Geodetic Survey.)

to glance at the chart for the period of 1905 and compare it with that of 1910 to see that the additional readings made in five years greatly change the map. Twenty-five per cent. more readings were made in the period between 1905 and 1910.[1] The details of the 1910 chart are far better than those of the 1905 chart (see Fig. 75). There are a great many more irregularities in most of the states. This increase in irregularity is due to the greater number of readings in a given area. The following table shows the number of square miles per magnetic station in the United States for the periods of 1905 and 1910 (see Table I.).

TABLE I.

TABLE SHOWING THE NUMBER OF SQUARE MILES OF TERRITORY COVERED BY EACH MAGNETIC STATION IN THE UNITED STATES FOR THE PERIODS OF 1905 AND 1910.[2]

	1905	1910		1905	1910
Alabama	1,306	1,026	Nebraska	1,230	817
Arizona	1,430	1,394	Nevada	1,757	1,743
Arkansas	1,417	921	New Hampshire	716	475
California	1,228	1,131	New Jersey	174	150
Colorado	1,823	1,672	New Mexico	1,679	1,678
Connecticut	151	115	New York	572	364
Delaware	158	141	North Carolina	443	390
Florida	826	722	North Dakota	2,145	1,462
Georgia	929	618	Ohio	419	388
Idaho	2,924	2,539	Oklahoma	2,012	1,246
Illinois	1,574	514	Oregon	1,334	1,226
Indiana	1,173	443	Pennsylvania	551	498
Iowa	1,273	598	Rhode Island	114	97
Kansas	714	687	South Carolina	650	622
Kentucky	546	402	South Dakota	2,353	1,671
Louisiana	518	473	Tennessee	765	474
Maine	688	409	Texas	1,460	1,360
Maryland	90	64	Utah	2,296	2,163
Massachusetts	173	146	Vermont	736	507
Michigan	701	340	Virginia	363	294
Minnesota	1,895	763	Washington	760	668
Missouri	463	360	West Virginia	427	353
Mississippi	1,115	1,030	Wisconsin	1,192	588
Montana	2,519	2,278	Wyoming	2,083	2,033

The average number of square miles for each station in 1905 was 973. On the chart of 1910 the average was 756 square miles. a decrease of twenty-five per cent.

[1] "Distribution of the Magnetic Declination in the United States for Jan 1, 1910," Special Publication No. 9, p. 3, Dept. Commerce and Labor.

[2] *I. e.*, Special Publication No. 9, Coast and Geodetic Survey.

FIG. 75. Map showing the lines of equal magnetic declination for 1905 and 1910 and how the lines have changed in five years. Note the greater irregularities for 1910 than for 1905. (After charts of Coast and Geodetic Survey.)

There is a direct relation between the irregularities in each state and the number of square miles to each magnetic station. The greater the number of observations, or the smaller the number of square miles to each station, the greater the irregularities. This is very forcibly shown in the western states, where there is a large area to each station (and these stations are not distributed equally geographically). The isogonic lines are much more regular than in the eastern part of the United States where the average area per station is much less (see Table II.).

TABLE II.

TABLE SHOWING THE NUMBER OF SQUARE MILES PER STATION IN THE EASTERN AND WESTERN PART OF THE U. S.

	1905	1910
States east of the Mississippi River	654	423
States west of the 102 meridian	1,766	1,684

A still better example of the relationship between the number of square miles per station and the irregularities in the isogonics is shown in Wisconsin. On the chart of 1905, there are 1,195 square miles per magnetic station and in 1910 there are 588 square miles per magnetic station. The variations in the isogonic lines in the chart for the last period are very much greater. The same feature is seen in Minnesota, where on the 1905 chart the average is 1,895 square miles per station and on the 1910 chart it is 763 square miles. Other states that show marked changes due to more observations are Indiana, Illinois, Iowa and Michigan.

Two readings of nearly the same value but widely separated in longitude furnish another cause of marked irregularities. This causes the isogonic to make a deflection out of its normal course. This same deflection usually disappears on the next chart, showing that the cause of the deflection was only temporary and certainly did not arise from permanently magnetized masses in the earth's crust. The three swings of the isogonic of 5° E. Decl. (chart of 1905), one to the east in western Kentucky, another

to the west in N. E. Arkansas, and the last to the east again in southern Tennessee, illustrate this. On the 1910 chart, the one in southern Tennessee is still present but shorter, the deflection into N. E. Arkansas has actually been reversed and extends slightly to the east, while that in western Kentucky has disappeared. Many other examples are evident from the charts.

Two abnormal values in the same latitude also produce marked sinuosities. On the 1905 chart in S. W. Nebraska, a declination of 13° E. Decl. is seen, while in the same latitude and one and one half degrees to the west a local declination of 11° is found. The result is evident from the chart. These abnormal values have disappeared in the 1910 chart and the region presents no marked variations.

Local abnormal values (shown by the enclosed areas) are very abundant on the 1910 chart, there being a great many more than on that of 1905. The numbers of each are 122 and 11 respectively. Many of these values are strikingly abnormal, for example where the St. Lawrence flows out of Lake Ontario a value of 37° occurs. There are many such occurrences around Lake Superior, the variation being from 1° to 18°. The explanation of the abnormal values is no doubt to be found in the greater number of observations which have been made. It will be remembered that in Minnesota and Wisconsin the number of square miles per magnetic station was reduced about one half on the 1910 chart, showing that the number of the determinations of the magnetic elements had been doubled, and this confirms our conclusion that more observations will show more anomalies and more irregularities.

There is a remarkable anomaly near Kursk, Russia.[1] The area is about 4 square kilometers (1.54 square miles). It is in a rolling country, varying in altitude between 146 and 310 meters, and is covered with a fertile, black soil resting upon Cretaceous rocks. It is located in longitude 51°, latitude 34°, east of Paris. In this limited area the declination varied from 34° E. to 96° 37′ W.; and the inclination varied from 48° to 79°.

[1] Littlehales, G. W., *Terr. Mag.*, Vol. IV., p. 235.

These values correspond to the normal inclinations of Morocco and Spitzbergen. The intensity showed even greater variation from the normal. In April, 1898, a point was found where the dip was vertical and the compass needle lost all directive force. Borings over 200 meters in depth failed to reveal any traces of iron. Anomalies may exist but they do not prove that masses of iron are below.

The geographic distribution of the magnetic observations is not uniform. Certain regions which are difficult of access will not have as many observations as those where traveling is easy. This unequal distribution will cause local variations.

We thus see that with more magnetic stations there is an increase in the irregularities in all regions. And this would suggest that when the magnetic survey of the United States has reduced the number of square miles per magnetic station to a few miles instead of hundreds of miles as it is now, it will be difficult to find an area differing greatly from the other areas. And with the increase in the number of stations will go a corresponding decrease in the prominence of small local variations.

A local magnetic disturbance deflects the horizontal needle from the true magnetic meridian in the direction of the disturbance. This deflection should be towards the disturbance from both the right and left. This was actually found to be the case by Rucker and Thorpe in England. The north end of the needle was attracted towards the area where the dip was at a maximum.[1] The maximum deflection should be at right angles to the zone of maximum disturbance. Since the maximum intensity of the horizontal force is located at the point where the angle 35° 16′ emerges at the surface,[1] if the mass of iron were large, as it must be according to Dr. Becker's interpretation of the chart for 1905, this zone would be irregular in extent and in intensity, yet the total effect on the isogonics would be to deflect them inward toward the magnetic mass.

A careful study of the charts for the periods of 1905 and

[1] Rucker, A. W., *Terr. Mag.*, Vol. III., pp. 42–43.

1910 does not reveal a single instance of such a narrowing of the isogonics in any locality. Instances are found where two or three isogonics appear to have been drawn together but these are no doubt due to exceptional values of the declination and in many instances disappear on the next chart. None which occur in the vicinity of oil fields could be assigned to permanent magnetic masses.

Are we justified in making such a statement as is quoted at the beginning of this paper when it is based on a chart that is so noticeably changed by the additional work of a few years?

The following evidence is offered to show that no relationship exists between the variations in the isogonics and the location of the existing petroleum fields.

1. The isoclinals do not show any evidence of disturbances from magnetic masses.

2. Secular variation is a shifting of the isogonics, which would indicate that the origin of the variation is not due to a stationary mass.

3. The magnetism of iron is lost at high temperatures, hence it must exist at comparatively shallow depths to be magnetically effective.

4. The geological evidence is not strong.

1. THE LACK OF EVIDENCE SHOWN BY ISOCLINALS.

The isoclinals are lines which indicate localities where the magnetic dip is equal. If magnetic masses existed below the surface of sufficient size to influence the horizontally swinging needle, then these same masses most certainly would influence the dip needle which can only swing in a vertical plane. The following diagram makes it evident that at any time the force tending to deflect the declination needle from the true magnetic meridian is less than the force exerted on the dip needle (Fig. 76).

The force exerted on the declination needle at B is the horizontal component BC of the total force AB exerted at B. The dip needle is acted on by the component AC which is greater than BC. H. L. Smith has shown[1] that the declination is zero over a

[1] Smith, H. L., ECON. GEOL., Vol. II., p. 367, 1907.

magnetic mass (point C) and that it increases to a maximum at a certain distance and decreases again to zero at an infinite distance. The distance at which it is a maximum is 35° 16′. Thus at no time can the horizontal intensity of the magnetizing force of the mass equal that of the force attracting the dip needle.

The chart of equal magnetic inclination for 1905, published by the Coast and Geodetic Survey, fails to reveal any evidence of such variations. A certain amount of smoothing out of the lines is necessary in the construction of charts such as these used

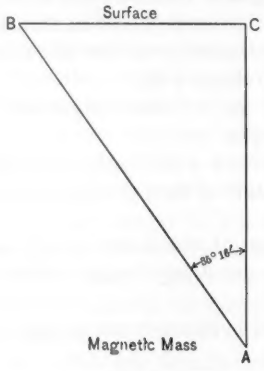


FIG. 76.

in showing the declination and inclination. The smoothing out of the lines is not enough to eliminate any evidences of deflections, if such existed. The writer found this to be the case on plotting the isoclinals for every fifteen minutes in the magnetite areas in New York and New Jersey, and for every thirty minutes in the states of Pennsylvania, Maryland, Ohio, Indiana, and West Virginia. In the New Jersey region the maximum variation from the normal is less than 1°. One reading near Mineville, New York, varying less than 1° from the normal, causes an eastern swing of the lines. This deflection can be seen on the Isogonic chart for 1905 in the Adirondack region. The variations are due to the magnetite masses near the surface.

It seems very strange that a mass of iron that would cause a deflection of the isogonics would not influence the isoclinals and yet there is no evidence of such an influence in any of the oil regions or elsewhere. It seems to be very strong evidence against the suggested relationship.

THE EVIDENCE OF SECULAR VARIATION OR CHANGE.

Secular change is the variation of the declination in any locality. This change is constantly going on and its direction is in general clockwise at the present time, although there may be temporary reversals of this.

If the irregularities of the isogonics are due to magnetic bodies in the earth's crust they should be preserved, as each isogonic occupies the same position over the permanently magnetized region. This could only be told by comparing charts of different periods. As the annual change in the United States is small, from zero to five minutes, the charts compared should belong to periods separated by fifteen to twenty years. The chart for 1905 is the earliest accurate chart which is based upon many values and that for 1910 the most recent, therefore the time interval has not been long enough for one to say definitely that the irregularities will or will not remain the same.

Nevertheless, a very good idea of what will take place can be gained from a study of the two charts of the periods mentioned. The shifting of the lines is not very noticeable in the Mississippi Valley where the annual change is near zero, but in the New England states and on the Pacific coast where the change is 5′ or 6′ annually, there is sufficient shifting to give us a clew to the probable future changes. The isogonic of 14° W. Decl., chart of 1910, has shifted until it is not far from the approximate position of the isogonic of 13° W. Decl., chart of 1905. Likewise some of the isogonics of the Pacific coast have changed their position and approached the position of the isogonic of the chart of 1905 near enough to show that the isogonics in general will not have the same sinuosities as those of the earlier period. In many instances the deflection of the isogonic on the 1905 chart is one or two degrees and the succeeding isogonic now has

a deflection of an equal amount in the opposite direction. This shows that the variations are local and are not due to any permanent magnetic force. The evidence, at present, seems to indicate a continual variation in the irregularities of the isogonics and does not favor the hypotheses of magnetic masses below. Dr. Bauer[1] states that secular variation is caused not only by a change in the direction of magnetization of the earth but also by a change in the intensity of magnetization and is due both to an internal and external system of magnetic forces standing in certain relation to one another.

THE LOSS OF THE MAGNETIC PROPERTIES OF IRON.

It is well known that iron when heated to a red heat loses its magnetic properties. Everetts[2] gives the following experiment. With a magnetizing force of 1.3 the permeability of an iron ring was 11,000 at 770° and only 1.14 at 785°. Nickel becomes non-magnetic at a temperature of 300° and cobalt at a bright yellow heat. The curve, Fig. 77, showing the magnetic permeability of iron with rise of temperature, is taken from Morris.[3] It shows the rapid loss of the magnetic properties of iron when it is heated to what Morris calls the critical temperature of iron.

If now we take the rate of increase of temperature as we go downward in the earth's crust and multiply the critical temperature by this rate of increase, we get some idea of the depth at which iron will lose its magnetic properties. Assuming the rate of increase of the temperature as 1° C. for 30 meters and taking the critical temperature as 780° as given by Morris[3] we get $780 \times 30 = 23{,}400$ meters or 23.4 km. (14.5 miles) as the approximate depth for these conditions.

Bidlingmaier[4] concludes that iron must be within 20–26 km. (12 to 16 miles) of the surface in order to be magnetic. Dr.

[1] Bauer, L. A., *Terr. Mag.*, Vol. 15, p. 110, 1910.
[2] Everetts, C. G. S., "System of Units," 1891, p. 150.
[3] Morris, *Phil. Mag.*, Vol. 44, pl. 12, p. 251.
[4] Bidlingmaier, Fr., *Physick. Zeits.*, Vol. XI., pp. 1216–1222, 1910.

J. A. Fleming,[1] quoted by Bauer,[2] estimates that the magnetizable substances must all lie inside of 10 or 12 miles of the surface. The increase of pressure downward would probably slightly increase the depth at which the demagnetizing temperature would be reached.

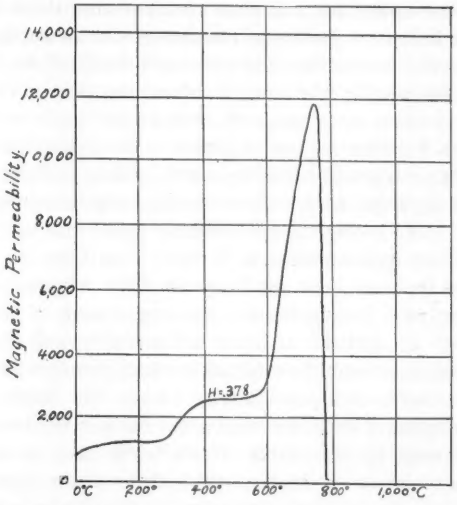


FIG. 77. Curve showing the rapid decline in the magnetic permeability when iron is heated above its critical temperature point. H = field strength.

Assuming the maximum depth to be from 12 to 16 miles at which we may expect to find magnetic substances, have we any way of knowing whether they occur in this zone? The evidence on this will be taken up in the next topic.

THE GEOLOGICAL EVIDENCE OF THE EXISTENCE OF IRON.

Geological evidence that iron exists in the upper few miles of the earth's crust is lacking. Dr. Becker has summarized the evidence on this point in his paper.[3] He found that metallic iron

[1] Fleming, J. A., *Terr. Mag.*, Vol. 2, p. 57.
[2] *Terr. Mag.*, Vol. 16, p. 37, 1911.
[3] Becker, G. F., Bulletin 401, U. S. Geol. Survey, p. 22.

could be detected in only six sharply determinable cases among the several hundred slides of rocks from the United States which he had examined.

In view of the extensive igneous activity in the United States it is remarkable that so very few occurrences of metallic iron were determinable, for one must conclude that there would at least have been some masses of iron brought to the surface by the igneous rocks, unless they are extremely localized in the crust.

Since the metallic iron must lie above the isothermal surface of 780° in order to be magnetic, and as this plane in all probability lies between ten and eighteen miles below the surface, it does not seem possible that the many igneous rocks could have passed through this zone without encountering occasional masses of iron. Most geologists will probably agree that the depth of origin of the igneous rocks is at least more than six or eight miles and that the great majority of them originate at much greater depths. Taking the average temperature of lavas as at least 1100° C., and the majority are probably well above this, this temperature would be attained at about twenty miles, where the igneous rocks exist potentially as a lava. No doubt there are igneous magmas at shallower depths but the fact remains that the origin of most igneous rocks is sufficiently deep to encounter any masses of iron which may exist above the magnetic limit. Then why do not igneous rocks contain more evidences of iron? It must be because there is none there.

Further, we have no right to exclude the possibility that iron may be brought up from below the isotherm of 780° C. If iron exists in the upper fifty or one hundred miles of the crust, there seems to be no doubt that some of our magmas drain from that depth, and yet the Greenland occurrence is the only one recorded in the whole world with more than a few scattered grains of metallic iron.

And why do not at least some of the three hundred active volcanoes show evidences of metallic iron if it is in the crust? They are certainly sufficiently widespread, geographically, at least to drain or pass through some areas where metallic iron

occurs. An investigation of the volcanic areas showed that there are active volcanoes in many of the oil regions of the world. And not a single volcano gives evidence of metallic iron.

One would expect iron to be found in the recent lavas and igneous rocks of the Western States. Certainly volcanic activity has been recent enough there so that some iron would have been still preserved from weathering. And even if it had oxidized the resulting products should be present in recognizable masses. But no evidence of such products has been found. In California the formation of the oil or even part of it should have freed metallic iron. According to Dr. Becker, this oil field, although there is the best kind of evidence of organic origin, was derived from carbides, and this iron should appear in some of the intrusives or extrusives of California.

It may be said that the lava from all the active volcanoes has not been examined. But nearly all the three hundred have been visited and if there were more than traces of iron present in the lavas it doubtless would have been observed.

It is of course well known that where magnetite rocks outcrop at the surface, they cause more or less of a deflection of the isogonics. This is readily seen in any of the regions where magnetite is known to occur. A. W. Rucker,[1] basing his statement on the large number of magnetic observations made in Great Britain, says: "where large masses of basalts exist, as in Atrim, in the Scotch coal fields, in North Wales, and elsewhere, the north pole of the needle is, as a matter of fact, attracted toward them from distances which may amount to fifty miles. The thickness of the sheets of basalt is in most cases too small to furnish a complete explanation of the observed facts, but it is quite possible that these surface layers of magnetic matter are merely indications of under ground protuberances of similar rocks from which the surface sheets have been extruded. At all events there is no possible doubt that where large masses of basalt occur, the north pole of the needle tends to move towards them."

As cited above there does not appear to be any particular de-

[1] *Terr. Mag.*, Vol. 3, pp. 42–43 (quoted from L. A. Bauer, "U. S. Mag. Decl. Tables and Isogonic Charts for 1902," p. 68).

flection of the isogonics in the regions of supposed iron masses. It seems remarkable that while rocks like basalts, with a very low magnetic property, can appreciably attract the needle at distances of fifty miles, there does not appear to be any marked deflection due to the postulated iron masses. And since these masses of iron must be in the upper ten to eighteen miles in order to be magnetic, they are certainly near enough to produce marked disturbances of the needle. Their effect on the dip needle should be even stronger, but the isoclinals do not show any such result.

Rucker and Thorpe have found that in general, when older rocks (Carboniferous or older) protrude through younger rocks, the needle is attracted towards them. The magnetism is no doubt due to the metamorphism which the older rocks have undergone, for it is well known that magnetite is developed under such conditions.

While one could conceive that the available carbides had been exhausted in the production of petroleum, it hardly seems possible that this is actually the case. Hence, we should expect to find the process going on now. As there is certainly a great area over the earth, where possible carbides might be broken up, why should there not be evidences of oil and gas in the Canadian Shield or in other pre-Cambrian areas? It certainly cannot be said that there is a lack of suitable reservoirs for accumulation in these areas. The work of Washburne in the Boulder oil field has removed all doubt that oil (and why not gas under certain conditions?) can accumulate in rocks without the historic porous formation, etc. Such joints and cracks occur throughout the Archean and Proterozoic rocks. Also a crushed, brittle rock could furnish ample pore space for the accumulation of oil. If oil is produced anywhere on the earth, there certainly are possible reservoirs where it could accumulate. Yet occurrences of oil in rocks of pre-Cambrian age have never been reported. E. Orton[1] many years ago called attention to the absence of petroleum in the Archean rocks of Canada and argued also that, under the carbide theory, its production should go on regardless of accumulation.

[1] Orton, E., *Bull. Geol. Soc. Am.*, Vol. IX., p. 85, 1897.

It does not seem possible that the carbide areas should always be under sedimentary beds, and we are probably more justified in asking the supporters of the carbide theory to explain the absence of all traces of petroleum in the pre-Cambrian, than they are in requiring us to account for the supposed connection of the irregularities of the isogonics and the petroleum areas.

CONCLUSION.

Finally, it is concluded that there is no connection between the lines of magnetic declination and the distribution of the oil fields

The present maps are not sufficiently accurate upon which to base such a conclusion because (*a*) the mapping is not in detail in all the areas, and (*b*) local abnormal values, which are temporary, cause great irregularities. These factors tend to localize irregularities and cannot be ascribed to stationary magnetic masses.

Isoclinals do not indicate the existence of magnetic masses.

The annual variation points to a marked change in the sinuosities of the isogonics, which would not be the case were they caused by stationary masses below.

Since iron loses its magnetic properties at 780° C. such masses must exist above the depth at which this temperature is reached, *i. e.*, between twelve to eighteen miles.

There is almost an entire lack of geological evidence that iron or magnetic masses exist in the outer portion of the crust.

USE OF GEOLOGY IN IRON ORE EXPLORATION.[1]

C. K. Leith.

I was asked recently, "Of what use is geology in exploration for iron ore?" My purpose is not so much to make a case for the geologist, as to discuss plans of exploration desirable from a geological standpoint. The regions to which principal reference will be made, for comparative purposes, are Lake Superior, Canada, Cuba, and Brazil.

EXPLORATION OF LAKE SUPERIOR IRON ORES.

Some sort of a geological survey of an area, professional, amateur, or otherwise, is now usually regarded as an essential preliminary to exploration for iron ore. The function of the geological survey for exploration purposes is to eliminate the barren areas. The Lake Superior ores are in certain definite iron formations of sedimentary type which can be followed by ordinary stratigraphic methods, and few now wish to spend money in exploration in an area of granite or quartzite, or other non-iron-bearing rocks. In the past this was not as true as it is now, as shown by the expenditures that have been made in such areas. So much of the Lake Superior country is drift-covered that the tracing of iron formations requires magnetic surveys and careful study of structure and stratigraphy in order to extend and connect the iron belts properly through covered areas. There is practically no phase of physical geology which can not be employed to advantage in delimiting the limits of the field of most promise for exploration. The geological surveys to date have eliminated all but about 270 square miles of territory in the Lake Superior region from the zone of favorable exploration. In other words, the area of iron formation known and inferred is about 270 square miles, distributed approximately as follows:

[1] Presented at meeting of Canadian Mining Institute, Toronto, 1912.

Area of known iron formation

	Square Miles.
Michigan	45
Minnesota	160
Wisconsin	20
Ontario, adjacent to Lake Superior	45

In addition to the above are many magnetic belts, partly or wholly unexplored, that may add another 100 square miles of iron formation.

The enormous preponderance of the area of Minnesota is due to the Mesabi range, where the beds lie nearly flat, as contrasted with the steeply dipping beds of other districts.

The geological examination still further narrows the field by eliminating from favorable consideration certain extremely magnetic and silicated phases of the iron formation which are known by wide and costly experience to be nearly barren of ore. These parts of the formations may reach from 40 to 45 per cent. of iron in narrow rich seams. They are hard and resistant, and are conspicuous topographically on the erosion surface. For half a century they have attracted the attention of explorers; as soon as one group of explorers have satisfied themselves that there is nothing in this type of formation there is another group ready to go through the same experience. The geologist has shown that this type of formation is a result of igneous or deep-seated metamorphism before the formation had the opportunity to be concentrated to ore; and that this kind of metamorphism has not itself concentrated ore bodies, and still more important, has put the formation into such condition that the ordinary surface agencies of concentration have later not been able to accomplish their work. It is not true that all magnetic portions of the iron formation are to be eliminated because of this, but nearly all of the extremely magnetic and silicated phases are to be eliminated. One of the commonest fallacies is that high magnetic attraction is a favorable indication of an ore body. Looked at from the standpoint of Lake Superior experience as a whole, a strong magnetic belt is an unfavorable indication for ore close at hand. In support of this statement I need only cite the slightly magnetic character of the

great hematite bodies of the Mesabi and other Lake Superior ranges, as compared with the highly magnetic properties of the lean east Mesabi, east and west Gogebic, and other parts of the Lake Superior ranges. A strong magnetic belt may indicate the existence of the iron formation and gives a valuable starting point in working toward less metamorphosed phases of the formation where there is better promise of finding commercial grades of ore.

The elimination of the magnetic and silicated phases of the formation means reducing the total area of iron formation in the Lake Superior region roughly by half, leaving about 135 square miles within which to explore. This may be increased in small measure by parts of the unexplored magnetic belts.

It is possible still further to reduce the area for exploration by discarding areas of iron formation which are known to be lean and "tight," that is, areas in which the formation has originally a low percentage of iron and in which the processes of loosening up by weathering essential to the concentration of ore have not been begun. Such areas are recognized in almost any producing district.

By processes of elimination requiring careful geological study at every step the explorer may come to a consideration of about 75 square miles of iron formation in the Lake Superior country.

Within the area thus localized the problem now is to find the parts concentrated to iron ore. Concentration is effected by water and the atmosphere, and accessibility of these substances to the iron formations determines the localization of the ores. There is the greatest variety of factors which determine the localization—whether the beds are flat or on edge, the existence of impervious basements or covers, faulting, jointing, nature of the folding, porosity, area of exposure to the surface, and other factors. If you stop to think of the variety of ways that surface solutions can get at rocks effectively you may appreciate in some measure the complexity of the conditions under which the ores may be found. Other things being equal, the concentration is roughly proportionate to the area of exposure of the iron formation to the erosion surface, all the remaining factors being subsidiary and modifying. This is something more than the state-

ment of an academic principle, for it has been worked out in terms of acres and tons of iron ore and iron formation for the different districts. Approximately 80 per cent. of the area of the Lake Superior iron formations is in the upper Huronian series and this series has produced about 80 per cent. of the total past shipments, and probably has more than this proportion of the remaining reserve. Regardless of the modifying factors, a certain percentage of the area of any iron formation may be expected to be ore. In the Mesabi district, after elimination of the non-productive east and west ends of the range, 8 per cent. of the remaining area of the iron formation is known to be ore. In the Vermilion district, between Tower and Gunflint Lake, 2.3 per cent. of the surface of the iron formation is ore. In the Michigan ranges, collectively, the present ore area is .6 of a square mile, or 1.5 per cent. of the favorable iron formation after elimination of obviously barren phases. But making allowance for ore area mined out and probable further discoveries the true percentage is probably nearer 4 per cent. The lower percentages of ore areas in the steeply-tilted iron formation of Lake Superior, as compared with the flat-lying Mesabi, dies not necessarily mean a smaller proportion of ore, for there are better chances of continuance with depth in the steep-dipping than in flat-lying beds, as I shall show later.

In explorations started on any part of the iron formation which is non-magnetic and non-silicated, from 2 to 8 per cent. of the drill holes, depending on the district, should penetrate ore. The unit of exploration is a 40-acre tract. At least this percentage of the 40-acre tracts, therefore, should show ore. As a matter of fact, particularly in Michigan, a very much larger percentage of the 40-acre tracts, possibly half of them, contain ore. In other words, the small percentage of ore area is distributed widely over the 40-acre tracts on the iron formation.

Careful geological study of any area being explored in detail still further limits the area of exploration. It will be found that on part of the forty acres the formation is richer than on another part; that is more leached and porous than on another part, that there are various structural features which favor one part

rather than another. Work started miscellaneously over the forty acres will gradually converge toward one spot. The rapidity and effectiveness of his convergence depends largely upon the extent to which the geology, the structural relations and origin of the ores are understood. I should like to enlarge upon this in more detail. We have attempted in our recent monograph on Lake Superior geology[1] to tell of some of these conditions. I would here merely emphasize the fact that no rule-of-thumb method or consideration of one or two factors will suffice. There is almost no fact relating to the structure or metamorphism, conditions of sedimentation, chemistry of deposition of the ore, factors entering into secondary concentration—no matter how apparently unrelated to practical exploration—which does not contribute to a comprehension of the possibilities in a given situation, and facilitate the finding of the ore.

After ore is found and the mine opened, intensive geological study is able still further to save much useless work.

In this advanced state of development there are again certain average results to be looked for which should be considered in scaling exploration or mining for the future. The Mesabi ores are easily explored in three dimensions and tonnage estimated. The necessity of inferences beyond ascertained facts is at a minimum. Most of the other ore bodies of Lake Superior show only their edges at the surface and adequate exploration in depth and consequent complete estimates of tonnage can not be expected for many years to come. Some facts assembled by the writer in connection with estimates of ore reserves of Michigan[2] for the Michigan Tax Commission in the past two years give a reasonable basis for expectation of large tonnage below present workings. In fact, when a depth has been explored comparable to the width of the Mesabi iron formation at the surface, it is not impossible that the tonnage of ore may be as great as on the Mesabi. This depth, however, may be too great for profitable

[1] "Geology of the Lake Superior Region," by C. R. Van Hise and C. K. Leith. Mon. 52, U. S. Geol. Survey, 1911.

[2] "Iron Ore Reserves of Michigan in 1911," by C. K. Leith. Advance chapter from "Min. Resources U. S. for 1911," Washington, 1912.

mining, and it may appear further that the percentage of formation altered to ore at depth is not so great as on the erosion surface. The method of forecasting greater depth of ores is as follows:

In order to make a common basis of comparing all ore deposits of varying structure and of different districts, the actual area in square feet may be calculated for fully developed bottom levels in ore in each of the Michigan mines. Knowing the total tonnage which has been shipped in the past above this bottom level and adding to it the tonnage in reserve above this bottom level, it is possible to calculate the total cubic dimensions of all the ore now or formerly in the mine above the bottom level. Dividing this cubic figure by the depth of the bottom level, an average *area* is determined for all the levels above the bottom level. This average area compared with the present bottom area gives a fair notion whether the mine is better or worse to-day than in the average of its history. In this method of calculation it appears that the total area of bottom levels of all mines in the Gogebic district with an average depth of 1,456 feet is 20 per cent. larger than the average higher levels mined in the past. In the Iron River district the present bottom levels of all the ore bodies in the district with an average depth of 690 feet aggregate over twice the area of the average higher levels. In the Crystal Falls district the bottom level at an average of 1,070 feet is about the same as the average levels above. In the Menominee district at an average depth of 1,160 feet, there is a falling off of about 30 per cent. from the higher levels. This is the only case of diminishing area in Michigan, but even this figure promises considerably greater depth to the ores. If the area shows no diminution and the ore bodies show no other signs of ending, it seems to me to follow that there is no fact against an expectation of future as good as the past in an average case.

These figures have to do only with the producing mines of Michigan. There is in addition an area of ore developed by drilling, not yet mined, of 9,000,000 square feet, which, as a matter of fact, is half again as much as the total area of ore in the bottom level of all active mines. This fact is of interest, as

showing how in the oldest part of the Lake Superior iron district exploration is still largely productive of results. I expect it to remain so for some time, not only in areal exploration, but in following ores down when they are found.

All of the foregoing figures are useful as checks or guides in forecasting results from large exploration plans. If the plans are comprehensive enough and are carried out for a long enough period, the results may be expected to approximate these proportions. I might state concrete cases in large scale operations in which geological work has at the start eliminated 80 per cent. of the area and has made still further eliminations during the progress of drilling until the later stages of work have been concentrated in the richer parts of the iron formations. In these later stages, where the work has been intelligently directed, from 2 to 8 per cent. of the drilling has been in ore. In opening up and mining the ore thus found advantage may be taken of the average facts of depth and persistence of ore bodies in relation to area, which I have described. Ultimate realization may fall short of this average in some properties, but it is likely to run over in others. I shall be surprised to see any wide departure from the average figure.

Of course there is much successful exploration which does not utilize these methods. If the exploration is followed far enough ore is found in about the average proportions already indicated. If lucky, the explorer may find it at once and not be at the necessity of using these geological inferences. But luck is not likely to be always on the same side in any long campaign of exploration. Where the explorer is limited in the area available to him, or limited in time or funds, his chances are less for approximating the above percentages, but whatever his chances are, they can be utilized to full advantage only by scientific work. In proportion as scientific work is ignored, his average chance, always barring luck, is reduced.

Elementary computation will show that if the explorer on any large plan of operations has a chance of getting ore in 5 per cent. of the area explored, there is an enormous percentage of profit awaiting him. The value of the ore found will, of course, show

wide range, depending on its quality, location, and many other facts, but notwithstanding this range the margin of profit makes the plan a reasonably safe one. A large profit is always looked for in mining, but with this large profit there is usually recognized an equally large hazard. The Lake Superior iron district offers one of the few opportunities of which I know for large profit with a hazard so diminished by large-scale scientific operations that it is possible to estimate with reasonable certainty the chances of finding ore. This is the basis for the widespread feeling that iron ore exploration on a small scale is a big gamble, and on a large scale is safe and profitable. There are very few large companies in the Lake Superior region that have followed plans of exploration consistently through a series of years without making a profit. Out of a possible twenty chances in twenty areas they take all twenty and on an average one to two out of the twenty is successful, giving them an ample margin. Certain individuals receiving large incomes from mining properties have found it profitable to set aside yearly portions of their incomes toward exploration, with a fairly definite notion of the percentage of return which could be expected in a long campaign of expenditure of this sort. The increasing use of geological knowledge for such campaigns of exploration may be regarded as the growing recognition of the possibilities of eliminating adverse chances by scientific work. If the geologist can eliminate only one or two of the twenty chances to be taken, more than the cost of his services is saved.

But, the question may be asked, how about actual discoveries made by geologists as compared with other explorers? As there is only one geologist to a hundred or more explorers, it is, of course, true that the geologist has not found as much ore as the latter, although cases of conspicuous success on the part of geologists may be cited. The real question, however, is not whether the geologist has or has not found most of the ore, but whether the use of geological knowledge has eliminated adverse chances to a sufficient extent to show a margin for operations so conducted over those of equal number conducted in less scientific fashion. I believe there would be no essential disagreement in answer to

this question. Now that the ore bodies have been found which outcrop or are otherwise easily accessible, geological inference is even more necessary than in the past, and we are likely to see an increasing use of this information.

The further thought may arise that it may be unnecessary to repeat this formidable programme of geological work for every piece of exploration. It is not necessary. The geologist has mapped the territory and set certain guide lines which can be ascertained in a few minutes by inspection of published maps. He has set the guide lines for understanding of structural relations and the origin of the ore. This is so much gained in taking up any plan of exploration, but there are few areas in which it is not necessary to know more about the distribution, structural relations, and origin of the ores, to arrive at a sufficiently close basis for exploration. A general knowledge will help, but it leaves a wide margin for random and unnecessary expenditure. Even if the published information in any area is reasonably complete the interpretation of this information and its application to a specific property are not always safely made by a non-technical man, and there is still room for the geologist with an understanding of a wide range of geological conditions to add a certain margin of safety and effectiveness to the explorations.

EXPLORATIONS OF IRON ORES OF ONTARIO.

Geological conditions similar to those of the Lake Superior region are repeated rather generally through Ontario and elsewhere in Canada. The general plan of using geology to concentrate explorations outlined for the Lake Superior region holds, so far as I know, for Ontario. It may be of interest to take stock of the present progress of exploration for iron ore in Ontario to see how far this plan has been followed, what remains to be done, and how definitely expectations can be stated. In locating and mapping the known iron belts the first step in elimination of non-productive formations is being rapidly taken, though much remains to be done in the vast unexplored tracts of Canada. The elimination of lean magnetic portions of the formations has been much less effectively done. I believe that there has been some

lack of recognition of this adverse factor. So far as I know the situation, at least a half, perhaps more, of the iron belts so far discovered in Ontario must be eliminated for this reason. Of the remainder somewhat more can be eliminated because of leanness and obvious lack of any effects of agencies of secondary concentration. There remains, however, a substantial residuum of iron formation in which there are reasonable chances for finding ore. Close structural studies have been made in but few places.

May the chances for ore be reduced to the percentage basis? Underground exploration has thus far been carried on to so small an extent that the ratio of ore to iron formation has not been established. We are compelled, then, to rely on analogy with similar formation in the Lake Superior region. It may prove that the chances of ore within the favorable iron formations approximate those of the Lake Superior region, namely, that ore occupies 5 per cent. of the area, but there is an adverse factor that in my judgment materially reduces this chance. The high average percentages of ore to iron formation of the Lake Superior country are based mainly on ores of the Huronian, which constitute over 90 per cent. of the Lake Superior ores. Iron formations of Huronian age thus far found in Ontario constitute only a small percentage of the total. They are principally of Keewatin age, which in the Lake Superior region contain less than 10 per cent. of the ore, and in which the percentage of area of ore to iron formation has been found by actual measurement to be 2.35 per cent. This comparison seems to me to throw light on the slow discovery of high-grade Ontarian iron ores, as compared with expectations based on the large areas of Ontarian iron formations and their assumed similarity to average Lake Superior iron formations. Recognition of the Keewatin age of many of the Ontarian iron formations now seems to me to reduce the chance for ore fully to that of the Keewatin iron formations of the Lake Superior region—in other words, to make the chances about half as favorable as in the Lake Superior region on an average. I do not imply that large quantities of ore will not be found in Canada. I think they will. I am merely trying to formulate some basis

of quantitative comparison with Lake Superior in order to determine what may be a reasonable expectation from any large-scale plan of exploration of Ontarian ores.

The careful detailed geological studies of small areas accompanying underground drilling remain largely to be done. When it is accomplished I anticipate results approximating the above figures.

EXPLORATION OF CUBAN IRON ORES.

I here refer to the great new reserve of medium and low-grade ores occupying so much of the area of central and northeastern Cuba, and not to the ore of quite different type which has been mined for many years in the Santiago district. The ores of northeastern Cuba are of such enormous quantity that they are likely to play no inconspicuous part in the world's future iron trade.

The use of geology on the northeastern Cuban ores may seem something of a post-mortem, in view of the fact that so much of the ore is exposed at the surface without even a covering of grass. As a matter of fact, however, the ore was first carefully examined and brought to the attention of the public by the geologists attached to Gen. Wood's staff during the first American intervention. Much more recently the gèlogical engineers of the large companies have played a large part in the location and delimitation of these fields.

The essential geologic fact is that the ores are surface alterations of serpentine rocks. It follows that the distribution of the serpentines must be ascertained. The fact that they are igneous rather than sedimentary rocks determines certain features of their distribution.

Within the serpentine areas the ores are localized by certain physiographic conditions. On the central plateau where the drainage is sluggish the ores may be fairly thick. At the edges of the plateau where erosion is active the ores have been nearly or quite removed. Where erosion has followed concentration slowly, the ores remain; where it has followed more rapidly, the ore is thinner or lacking. The problem then becomes one of con-

ditions and speed of erosion—a physiographic problem—contrasting in essential respects with the Lake Superior problems. The recognition of this relation of the ores to erosion surface by a local geological engineer made it possible to forecast the discovery of ore across a mountain chain in a difficultly accessible interior and led to one of the most recent large discoveries of ore.

Much remains to be done in correlating the ores and physiography. Other geological problems are: the reason for the locally increased depth of alteration of the serpentine to ore, the effect of climate and vegetation on the superficial enrichment and texture of the ore, and the interpretation of certain conglomerates in the ore of the Camaguey district to ascertain what part of the ore may represent concentration from an overlying limestone on the serpentine in addition to the concentration of the serpentine itself.

The chemical processes of concentration of the Cuban ores vary in no essential respect from those which have made the Lake Superior ores. The difference is that the concentration originated from serpentine rather than from iron formation and acted on all the area of the serpentine instead of being localized in small areas by complex structures, as it has in the iron formations of Lake Superior.

EXPLORATION OF BRAZILIAN IRON ORES.

There has been discovered in east-central Brazil, about 300 miles from the coast, an iron ore field more nearly resembling that of Lake Superior than anything before discovered, in grade, quantity of ore, area, geology, in probable pre-Cambrian age of the ores, and in sedimentary type of the ores. Here has been a fine opportunity to apply the geological principles developed in the Lake Superior region. The Brazilian field is not glaciated and much of the ore appears at the surface, but much of the field is covered with erosion detritus, more or less cemented, which conceals the geology, especially in the valleys, as effectively as does the drift in the Lake Superior region. There is the same necessity for careful stratigraphic and structural work in limiting the iron formations and inferring their extensions and connec-

tions. With such work it has been a comparatively simple matter to locate important ore bodies, which, if left to random exploration, might not have been discovered for many years.

A study of the origin of the ores discloses a fact of utmost significance in exploration, namely, that the rich ores are not local pocket concentrations in a great area of iron formation, but are, in fact, original iron formation sediments deposited in their present highly concentrated condition. In exploration it is not necessary to hunt for the local structural conditions favoring secondary concentration. The requisite is to follow out the entire bed, as one would follow a bed of quartzite. Somewhat the same uniformity and continuity and ultimate tapering out are to be expected both along strike and dip as would be found in a quartzite bed. I need not dwell on the enormous importance of this fact in the exploration for ore and especially in the estimation of tonnage.

There are, in this Brazilian field, additional but lower grade ores which clearly result from secondary concentration, and are analogous in every respect to those of Lake Superior. It is entirely possible that important deposits of this type will still be found, as the places favorable to their concentration are likely to be under valleys or slopes, which are now covered with detritus, requiring underground exploration.

The Brazilian field is one of the few fields where a large part of the tonnage—known to be as great as at Lake Superior—has been located and estimated largely by geological inference prior to commercial development.

CONCLUSION.

Having complimented the geologist under guise of describing exploration methods, it is now appropriate to admit that mining men have prospered well in the past and that some of them are now prospering, without the aid of geologists. I have a lively respect for the knowledge of mining men on geological facts in territory with which they are familiar. But as exploration is primarily a geological problem, it follows that the geologist should have a primary part in the work. The mining man man-

ages a vast variety of interests, often with marvellous success, but as an enterprise grows in size and complexity, specialization becomes necessary. In many cases specialists are placed in charge of the mechanical and civil engineering and railway operations long before it is thought necessary to place exploration in the hands of a geologist. So far as I have any argument to make in this paper (rather than casual comment on existing conditions) it is for recognition of the fact that exploration is mainly a geological problem and that exploration falls short of maximum effectiveness in proportion as it fails to use geological inferences.

In closing, I would say just a word about the reaction of mining and other commercial conditions upon geology itself. Brooks[1] has shown that in 1890 only 12 per cent. of the publications of the U. S. Geological Survey related to applied geology, while in 1909 this percentage had risen to forty-seven. There is no reason why applied geology should not be of the highest scientific type, but, as a matter of fact, much of it is not geology at all. To quote from Brooks: "There is, however, grave danger that, carried away by the present furore for practical results, we may lose sight of our scientific ideals. Applied geology can only maintain its present high position of usefulness by continuing the researches which advance the knowledge of basic principles. Future progress in applied geology depends on progress in pure geology." In the particular field I have here discussed it can not be too strongly emphasized that the scientific principles are the real basis of the aid to mining and exploration, and that too much time spent by the geologist in the application of principles rather than in development of principles must ultimately retard the work. After all the applications can be made to a large extent by the mining man or mining engineer. The understanding and development of the fundamental principles can be accomplished only by the geologist. I would plead that the geologist be allowed the fullest freedom in this direction, merely as a commercial consideration, and that he should not be confined, nor his work judged, alone by the direct practical results.

[1] "Applied Geology," by Alfred L. Brooks, *Jour. Wash. Acad. Sci.*, Vol. 2, No. 2, Jan. 19, 1912.

TRANSFORMATIONS AND CHEMICAL REACTIONS IN THEIR APPLICATION TO TEMPERATURE MEASUREMENTS OF GEOLOGICAL OCCURRENCES.

JOH. KOENIGSBERGER.

(Translated by JOSEPH A. AMBLER.[1])

I. POLYMORPHISM AND ISOMERISM.

The more exactly crystallized substances are investigated, the more often it appears that a given combination of chemical elements may exhibit many habits of crystallization which are stable within the limits of different temperature ranges. When substances are monotropic, the transformation takes place in only one direction, and it is then difficult to determine whether the several modes of crystallization correspond to different numbers of atoms or to different chemical compounds, *i. e.*, whether chemical isomerism and polymerism are involved, or physical polymorphism. The simpler the chemical compound, the greater this difficulty becomes.

This is best seen in the case of the elements, for the vapor, solution and liquid form can have a composition independent of the solid condition existing at the same time. The melting point, again, cannot be proved different in cases where one crystal form passes into the other before it has been reached, and the distinction between isomerism and polymorphism will then have lost its significance even by the theory of phases.

In spite of this difficulty, it is possible to consider chemical molecules of an entirely different constitution, as S_2 and S_3. In such cases the grouping of the molecules or their atoms as opposed to the chemical grouping of the atoms in the molecule will assert itself more strongly by the crystal structure.

[1] The original article in German of which this is a translation appeared in the *Neues Jahrbuch f. Min., Geol., u. Pal.*, Vol. XXXII., pp. 101–133, and is entitled "Umwandlungen und chemische Reaktionen in ihrer Verwendung zur Temperaturmessung geologischer Vorgänge."

A sharp transition point does not exist in monotropic substances, for, other things being equal, the higher the temperature, the more rapid the transformation. The same holds good for simple compounds, as FeS_2, SiO_2, etc., since they are monotropic. The transition point at which both are in equilibrium lies above the melting point, as W. Ostwald assumes. Could they be heated quickly enough before they were changed, two different melting points would be found, as for isomeres. Of the vapor, liquid, or solution, then, only one (or eventually another) chemical grouping is stable at a given time. It therefore follows that *the chemical difference* (*isomerism*) *in the solid condition is not to be distinguished from the physical* (*monotropic*) *polymorphism. It is not different even in principle.*

All sorts of transformations were conceivable by the phase theory, as, for instance, cases in which both of the chemical molecules are stable as separate liquids, but not as vapor, etc. It is therefore a matter of definition as to what is meant by polymorphism and isomerism. It is quite otherwise in the case of the reversible transformations, *i. e.*, the enantiotropic modifications, for here the influence of the chemical grouping manifestly predominates over the arrangement in the crystal framework. *Probably, however, every change of the one causes a more or less marked change in the other.* O. Lehmann[1] was the first to make this suggestion, and quantitative measurements by W. J. Müller[2] have confirmed it. These questions have been very thoroughly discussed recently by P. Groth,[3] even in regard to artificial crystals. There appear to be all sorts of transformations of enantiotropism to monotropism. For *pure* substances the following rule results from the data above presented: The more nearly the symmetry, principal planes, and physical properties of two crystal habits resemble each other, the smaller will be the difference in volume, the sharper will be the transition point, and the more rapid and easily reversible will be the change. In the case of the following substances the transformation occurs in less than 60

[1] O. Lehmann, "Molekularphysik," 1888–89, and *Zeitschr. f. phys. Chem.*, 83, 598 (1910).

[2] W. J. Müller, *Zeitschr. f. phys. Chem.*, 31, 354 (1899).

[3] P. Groth, "Einleitung in die chemische Kristallographie," Leipzig, 1904.

minutes at 5° above or below the transition point. Examples: Leucite[1] α and β at 560°, pyrite[2] α and β at 350°, boracite[3] α and β at 265°, quartz[4] α and β at 575°, silicon[5] α and β at 210°. This is easily explained kinetically. Every crystal form corresponds to a configuration of the atoms and molecules which is stable within a certain range. The heat change corresponds to Lagrange's virtual disarrangement. If the point of equilibrium is approached, the molecule, in transforming or rearranging itself under influence of the heat change, encounters only a small resistance and a weak reacting force. But if one molecule has changed into the new, stable conditions, the other molecules will also change. Isomorphous impurities which cannot fit exactly into the crystal framework of the pure substance will hinder the change. Thus, F. Rinne[6] found that boracite containing iron shows no sharp transition point, but a transition interval. The substances belonging to this division are especially adaptable as geological thermometers on account of the sharp transition point, but the small difference of the external forms makes the distinction of the two modifications difficult, and the other indications of change must be observed in order that the determination may be made with certainty.

2. There are substances much more inert, but still easily reversible, if the device be used, either of powdering the substance very finely or of adding a liquid with weak solvent powers; for example: sulphur α and β at 95°, ilmenite[7] α and β at 215°, titanium[7] β and γ at about 570°. In these cases the change in volume is mostly greater ($0.005 < v_1 - v_2 < 0.04$) and the external forms of the kinds of crystals are different; there appear no twin formations which effect an approach to the crystal forms of both modifications. If, however, the elastic forces to be overcome be minimized by powdering, so that the change is rendered

[1] C. Klein, *Neu. Jahrb. f. Min., Geo., und Paläo.*, 1884, II., 49.
[2] J. Koenigsberger and O. Reichenheim, *Neu. Jahrb. f. Min., Geo., und Paläo.*, 1906, II., 46.
[3] E. Mallard et Le Chatelier, *Bull. Soc. Min.*, 6, 122.
[4] Le Chatelier, *Compt. rend.*, 108, 97 and 1047, 1889.
[5] J. Koenigsberger and K. Schilling, *Ann. Phys.*, 32, 190, 1910.
[6] F. Rinne, *Neu. Jahrb. f. Min., Geo., und Paläo.*, 1900, II., 108.
[7] J. Koenigsberger and K. Schilling, *l. c.*

easy, or if the new structure be brought about by separation from solution, then the reaction is reversible. The higher the temperature above the transition point, the more rapidly the change takes place. This is true both because the stability of the β modification is greater and because the increased heat renders this, like all reactions, more rapid. For the change in the reverse direction, the two causes oppose each other, according to St. Claire Deville's principle of hot and cold tubes. The speed of reaction decreases rapidly as the stability of the α-modification increases; therefore the transformation at lower temperatures proceeds more slowly or not at all. This is shown very well in the case of ilmenite.[1] From α to β at $215° + 5°$, the change of 90 % requires about 1 to 2 hours. At $215° - 5° = 210°$, more than a day is needed before β is changed into α to the extent of 80 %. At 20° this change requires several months, as we have determined.

When these substances are used as geological thermometers, those with the greatest volume differences yield the most satisfactory results, because the reversible transformation leaves in the transparent crystals very clear traces, which manifest themselves in the crystal form.

3. Among the natural minerals, numerous instances of those with monotropic forms occur in which the change was made in one direction only. When such minerals are complex, it is doubtful if a distinction between polymorphism and isomerism is possible or necessary. Organic isomeres are altogether different, though for secondary reasons only. Owing to the high melting points of the silicates, both compounds can seldom be raised to their melting points, and on account of the great speed of reaction and ionization the liquids will always have the composition of the most stable compound.

In the case of two chemical compounds with the same atoms, a distinction may be made between that compound with a greater amount of free energy and that with a less; the former is necessarily less stable than the latter. The amounts of free energy may be dependent on the temperature, and a point may well be

[1] J. Koenigsberger and K. Schilling, *l. c.*

conceived at which the curves of free energy will intersect; this point would correspond to that of a reversible transformation of two compounds, and is in accordance with the facts of enantiotropic polymorphism. If this point lies above the melting point, the one compound will be more stable throughout the entire range of temperature. The higher the temperature, the more readily the transformation to this stable form will proceed. Reversible transformations of two chemically isomeric substances have been determined by O. Dimroth *et al.* in amino derivatives of triazolcarbonic acid esters. But in solution there exists an equilibrium dependent on the temperatures. This conduct of two isomeres corresponds to the assumption of W. Ostwald for monotropic polymorphism. In both instances no sharp transition point can exist in strictly monotropic changes; it can only be said that the higher the temperature, the more easily the transformation into the stable modification takes place. With those substances which have high melting points, the distinction between polymorphism and isomerism seems to me unprofitable; the difference is *gradual* not fundamental. The greater the difference of the crystal forms, the more different is the arrangement of the atoms in the chemical molecule, or their number. P. Groth[1] has shown that for molecules in the crystal framework our conception of the chemical molecule is inexact. In the case of somewhat complicated structure, on the other hand, it is very probable that there is actually a difference between the chemical molecules which constitute the crystal framework.[2]

Whether the difference between two so-called physical-polymorphic crystals remains constant in the vapor, or in the liquid states, or in the chemical reactions, depends upon circumstances, temperatures, pressure, etc. Thus calcite and aragonite give, in the two reactions of W. Meigen, different products, although, dissolved in hydrochloric acid they show identical properties.

The difference between feebly reversible and strictly monotropic substances ought to be fundamental. The first has an

[1] P. Groth, "Chemische Kristallographie," I., p. 172, 1906.

[2] This explains, for example, the conduct of the optical isomeres, the racemate, etc., in crystal, melt, and solution.

actual transition product; the second, an imaginary one above its melting point. But practically, the distinction between these substances is very difficult to make. The largest difference of volume, also great crystallographic and physical differences of both modifications, are common to them. The change makes a complete, easily recognizable destruction of the crystal. Therefore, a more certain maximum value is given by the temperature at which the *direct* change of the whole crystal is instituted. On the other hand, an extrapolation from values indirectly obtained is uncertain.

Likewise, in the feebly reversible, as well as the monotropic substances, the transition does not occur at a definite temperature, but through an interval which is theoretically of indefinite length; the velocity alone is dependent on the temperature. Probably v is determined by an exponential law of the form $v = c.\ T^n$, where n must be greater than 10. Apparently, therefore, there is a limiting temperature which cannot practically be exceeded. At very low temperatures, even the unstable form is, on the one hand, apparently stable, because there the time of the change becomes very great. Thus, for example, anatase and rutile exist side by side millions of years without change, as also quartz with tridymite, pyrite with marcasite, sillimanite with cordierite, augite with hornblende, calcite with aragonite. This may also be taken as a proof that during this interval they have never attained a very much higher temperature. On the contrary, tridymite in the "Euganeen trachyte," according to the investigations of Mallard[1] and R. Brauns,[1] has changed to quartz. This indicates a very slow cooling, or a second heating. In general, the unstable form will persist if the chemical reaction or the crystallization proceeds rapidly. This emphasizes the close relation between isomerism and monotropic polymorphism.

It is difficult to determine whether any one of the changes cited in section 3 for the polymorphs is reversible or not. The case tridymite to quartz is later discussed, and, speaking broadly, in no other instance is the reversibility determinable.

[1] See literature by C. Hintze, "Handbuch d. Mineral," 2, 1452.

In 1906, A. L. Day and E. S. Shepherd,[1] C. Doelter,[2] and the writer,[3] simultaneously alluded to the significance of *transformations for the measurement of geological temperatures.*

II. THE TRANSFORMATION OF THE α- INTO THE β-MODIFICATION OF QUARTZ AS A THERMOMETER.

Le Chatelier[4] has ascertained optically and dilatometrically the change of α-quartz, stable at ordinary temperatures, into a second β-modification. O. Mügge[5] made clear the simultaneously occurring crystallographic change and has referred to its importance for temperature determinations of geological occurrences. Fr. E. Wright[6] and E. S. Larsen, Fr. Rinne[7] and R. Kolb have studied thoroughly the changes of the optical constants. Wright and Larsen have fixed exactly the transition temperature at 575° for atmospheric pressure and have ascertained by careful, systematic investigations which type of quartz crystal develops in nature below the transition temperature and which above that temperature. They find that quartz from geodes, metallic veins, quartz pegmatites,[8] and pegmatite veins must have been formed below this temperature. The transformation appears to proceed instantly and spontaneously at atmospheric pressure, but it might naturally be questioned whether a change in pressure might not produce a great change in the temperature. Quartz, for example, in small fissure veins in granite pores, ore veins, and pegmatites, must develop in the presence of large amounts of water and carbonic acid. This can be shown from inclusions and their crystallization. The density of the water must be at least 0.3, because

[1] A. L. Day and E. S. Shepherd, *Amer. Jour. Sc.*, 22, 1906.

[2] C. Doelter, *Tscherm.'s Mitt.*, 25, 79, 1906.

[3] J. Koenigsberger, *Neu. Jahrb. f. Min., etc.*, 1906, II., 45. (Here also is allusion to the significance of the change of α- into β-quartz, discovered by Le Chatelier for the temperature of formation, p. 48.)

[4] Le Chatelier, *Compt. rend.*, 108, 97 and 1047, 1889.

[5] O. Mügge, *Neu. Jahrb. f. Min., Geol., und Paläo.*, 1907, 181.

[6] Fr. E. Wright and E. S. Larsen, *Amer. Journ. Sc.*, 27, 421, 1910.

[7] F. Rinne and R. Kolb, *Neu. Jahrb. f. Min., Geol., und Paläo.*, 1910, II., 133.

[8] Attention should be called to the fact that it is chiefly the ends or outer surfaces of the crystals in the pore spaces that are under consideration.

experiments have proved that the solubility diminishes very rapidly with the density. For temperatures between 400° and 600°, therefore, pressures of about 200–400 atmospheres are essential.

According to Clapeyron's formula,[1]

$$\frac{dT}{dp} = \frac{T(v_2 - v_1)}{q} \cdot 2,43.10^{-2},$$

the rise of temperature, dT in C for an increase of pressure of 1 atmosphere can be calculated; but $v_2 - v_1$, the difference of the volumes in condition 2 (at the higher temperature) minus volume 1 (at the lower temperature), must be known, as well as q, the quantity of heat absorbed. (q is positive if heat is absorbed by the change from 1 to 2.)

The latent heat of transformation, according to approximations by White,[2] is about 4 cal. per gram. The change of volume, $v_2 - v_1$, is found to be about 0.003 from the coefficient of expansion determined by Le Chatelier. From the formula $\frac{n^2 - 1}{s} =$ constant, applicable in all cases for the first approximation, $v_2 - v_1 = -\frac{2dn(n^2 - 1)}{s}$ where dn is the change of the index of refraction by change from 1 to 2, s the approximate density, n the approximate index of refraction for a color (best for one of large wave length). The measurements of Rinne and Kolb gave thus for $v_2 - v_1$, 0.0023. We take 0.0025 as the mean of both numbers and obtain $dT = 0.01°$ per atmosphere, which gives only 5° for 500 atmospheres. Hence, the quartz in the pegmatite masses must also have been formed below 580°.

In the Alpine mineral veins, the temperature also, as already shown in the place cited, was certainly lower than 580°. The quartzes, accordingly, show the surfaces characteristic of the tetartohedric α-modification and the inequalities of the rhombohedron. The investigation of the liquid inclusions in quartz had

[1] 1 indicates α-quartz; 2, β-quartz.

[2] Wright and Larsen, *l. c.*, p. 435.

previously made a temperature of about 250°–350° seem probable to us. We may attribute to the change of α- into β-quartz the property of a reliable geological thermometer.

III. THE TRANSFORMATION: QUARTZ — CRISTOBALITE — TRIDYMITE AS A THERMOMETER.

P. Quensel[1] and simultaneously A. L. Day[2] and E. S. Shepherd have determined that quartz is the form of SiO_2 which crystallizes in laboratory experiments below about 800°, and tridymite (or, according to the latest statement of Fr. E. Wright,[3] cristobalite) is the form which crystallizes above 800°. If the transition temperature is taken from this, the determinations of Day and Shepherd place it for atmospheric pressure at 800°. From Clapeyron's formula and the data submitted,[4] I estimate the influence of pressure just as J. H. L. Vogt has done, namely, at about 7° for 100 atmospheres, or 16° for one kilometer of depth; *it is, therefore, practically without significance.* Assuming the results of Day and Shepherd as most nearly correct, the following questions arise: 1. *Can unstable forms of SiO_2 be formed in nature?* 2. Can these exist without change?

The first question must be answered unconditionally in the affirmative, according to the observations in division 4. It is further probable that the range of existence is much more increased by pressure than the range of stability.

The second problem has been already discussed in section 1. The higher the temperature, the more rapidly that form which is unstable at a given temperature will pass into the stable one; but the time required will be wholly different according to the compound.

It is probable that quartz can thus crystallize out in nature above 800°; perhaps, as P. Quensel assumes, even up to 1000°.

[1] P. Quensel, *Centralbl f. Min., etc.*, 1906, p. 728, October.
[2] A. L. Day and E. S. Shepherd, *Amer. Jour. Sc.*, p. 266, 1906, October.
[3] Fr. E. Wright and E. S. Larsen, *Zeitschr. f. anorg. Chemie*, 68, 341, 1910.
[4] The differences of density at 800° are of the same order of greatness as at room temperature. The heat change will certainly not be smaller than that of α- into β-quartz.

The only uncertainty is the possibility of its persisting as such during the long cooling of the rock. Day and Shepherd[1] assert explicitly that a perfect quartz crystal shows no change after being 6 hours at 1400°. Therefore, it is entirely possible that the change will proceed much more slowly than the cooling of the rock at 1000°. This is even more true, of course, at 900°, especially if the pressure be somewhat higher.

Nature has placed before us an experiment of this kind. Quartzite pebbles at Brevik, in Sweden,[2] have been inclosed in a diabase flow.

FIG. 78. Diabase intruded into quartzite conglomerate (mag. about 50 times). (Brevik, southern Sweden.)

In the microphotograph, Fig. 78, the liquid magma *D* may be seen to have penetrated into the most minute fissures of the quartz pebbles (*a*). Whether or not a chemical reaction between the fluid diabase and the quartz has taken place here and there is immaterial. Suffice it to say that the chemically unaltered quartz pebbles, surrounded by the liquid diabase, must necessarily have assumed its temperature by virtue of radiation.

I have sought in vain for tridymite and cristobalite on the margins. They are both extremely easy to recognize, owing to the low indices of refraction (less than 1.490 for *D*-line) according to the method of separation given by Becke. The low double

[1] A. L. Day and E. S. Shepherd, *Amer. Jour. Sc.*, p. 274, 1906.

[2] "Guide Congrès géol. intern.," Stockholm, 1910, No. 18. A. G. Högbom, A. Gavelin, H. Hedström, p. 47. This interesting disclosure was made very kindly by the leading Swedish geologists to the participants in the expedition into southern Sweden.

refraction is, furthermore, very characteristic. The fluidity of the diabase shows that it must have possessed a temperature higher than 800°; according to the experiments of Barus, it was probably from 1000° to 1200°. Barus and Doelter give the melting point as between 1100° and 1200°. The temperature of solidification cannot be much lower than that in nature; for diabase, in contrast to the acid lavas, does not congeal as a glass at its surface, or only partially so, but behaves exactly as it does at greater depths. Mineralizers, therefore, which must escape at the surface, could not lower the temperature of crystallization. In consequence, the quartz has persisted as such, without changing into tridymite or cristobalite, in lava whose temperature was at the least more than 1000°.

The presence of quartz in deep-seated rocks cannot of itself be considered as proof of the possibility of its existence above 800°. In these rocks, it is the last mineral; mineralizers like fluorine compounds and CO_2 are undoubtedly present. A low crystallization temperature for quartz would, therefore, be conceivable in the deep-seated rocks.

It would be more difficult to bring the quartz in volcanic rocks into harmony with the assumption of its production below 800°. Glassy products, like obsidian, etc., are, of course, found at the surface of acid magmas where erosion has not yet removed the topmost crusts. The more acid the rock, the thicker the glassy layer. A. Lacroix[1] has observed that the newly built cone of Mont Pelée solidified to a glass on the outside. Beneath, tridymite-bearing andesite was first found, and only below this did the quartz-bearing andesite form. Lacroix attributes this, as does the writer, to the length of the cooling (page 155) and further to the influence of mineralizers, particularly water (517 et seq.)

The quartz-bearing rocks, of course, and particularly is this true of the quartz porphyries, are *not really surface rocks* like the quartz-free andesites, diabases, basalts, etc. They always require a moderate depth.[2] In my opinion, this does not accord

[1] A. Lacroix, "La montagne Pelée," p. 154 *et seq.*, Paris, 1905.

[2] The quartz porphyries of Carboniferous and Permic time have solidified, for example, most probably, about 50–100 m. from the surface.

with devitrification brought about by later action; for glassy, volcanic rocks from old geological periods are known to have persisted at the high temperatures and pressures to which they have been subjected in spite of the depth. Solidification to a glass is due in part to the absence of mineralizers which escaped to the surface, but chiefly *to a quick cooling.* The latter increases rapidly with the depth, but diminishes near the surface. A simple calculation[1] shows that, for an acid magma at 1 m. depth, 12 days are required for cooling from 1100° to 750°; at 10 m., 3 years; at 100 m., 300 years; at 1,000 m., 30,000 years. This holds true for a very extensive stream of obsidian, if no crevices, fissures, etc., develop. Actually, the time will be shorter, for the stream will be cooled from at least two sides, top and bottom. The figures here given are therefore only maximum values, but serve to give a clear idea of the process. In many *quartz-porphyries,* quartz is the *first* mineral separated out. After it, the orthoclase is crystallized out; then the plagioclase of the ground mass. It can scarcely be maintained, however, that these last minerals are separated out below 800° from a practically water-free magma. In my opinion, quartz could be crystallized out in the laboratory from the acid melts at from 1000° to 900° under pressures of from 20 to 50 atmospheres, provided only it were cooled sufficiently slowly (say, from 2 to 4 weeks). The analyses of A. Brun and his observations of fumaroles show that chlorine and sulphur compounds, as well as carbon dioxide, should be chosen as mineralizers. These should be present in

[1] There is the formula

$$\vartheta = 1100° \frac{2}{\pi} \int_0^{\frac{z}{2a\sqrt{t}}} e^{-x^2} dx,$$

$$a = 0.063 \frac{\text{cm.}^2}{\text{sec.}},$$

for obsidian, and from which t is to be calculated, for example, for a cooling to 750° (table of probability integral). From the formula resulting from the above, $t° = \left(\frac{z}{0.09}\right)^2$ (for obsidian).

small amounts, corresponding to those found in nature, namely, 500 c.cm. at 0° and 760 mm. per kg. of rock. The most important condition would, of course, be that of very gradual cooling from 1200° to 750°. According to the above calculation, the duration of this cooling should be at least a number of weeks.

Water is not a mineralizer or catalyzer. This has been shown by W. J. Müller and the writer.[1] A. Brun has shown that it occurs in many acid volcanic rocks only in small amounts or not at all. Tridymite as well as quartz may develop from glassy aqueous solution. Its formation depends entirely on the presence of mineralizers, and particularly the rapidity of the cooling.

From the above discussion, the following inferences may be derived:

According to the experiments of Day, Shepherd, and Quensel, the *range of stability* of quartz lies possibly below 1000°, but the range of its *existence* in nature extends higher, perhaps, as Quensel has determined from laboratory experiments, even to 1050°. It may also be possible that the range of stability extends to 1050°.

In general, the transformation of *tridymite or cristobalite* into quartz does not appear to take place *per se*. Tridymite is found in nature as a constituent of very old volcanic rocks. Only indirectly, by the aid of a fluid mixture of KCl and LiCl, which evidently dissolved the tridymite and separated out the quartz, have Day and Shepherd been able to obtain quartz from tridymite. The results were then obtained in a very interesting and careful series of experiments at a temperature of 720°. It is, therefore, noteworthy that exactly these substances in water solution are mineralizers, and always favor a separation of quartz.

On the other hand, rather long heating even at high temperatures, provided they be below 1000°, appears to have no influence. Those rocks in which tridymite and cristobalite separate out in cavities, were surely at a high heat for many years,—if not at 800° at least over 300°. The change of quartz to tridymite may perhaps be monotropic.

[1] W. J. Müller and J. Koenigsberger, *Centralbl. f. Min., etc., l. c., Neu. Jahrb. f. Min., et* Vol. 32.

The occurrence in extrusive volcanic rocks of *inclusions of quartz-bearing rocks, particularly quartzite,* which were torn away in depth by the molten magma, deserves careful consideration. A. Lacroix, in his work "Les enclaves des roches volcaniques,"[1] gives many such examples in basic and acidic rocks. The quartz always persists as such; only on the margins has change or alteration taken place. This holds good as well for basic as for acidic rocks. If it were true that quartz was not stable above 800° for any length of time, then these uprising magmas, even in depth, could not have had a temperature above 800°. This is very improbable. It is much more likely, in my opinion, that the temperature of the magma was below the limit of existence of quartz (about 1100°). That it lay above the range of stability, of which the lower limit cannot be definitely given, is very probable.

The marginal changes are, according to Lacroix: "melting = fondu à sa periphérie." This "melting or *Frittung,*" which would require a temperature of over 1400°, I would rather look upon as a chemical solution and solidification as an acid, but alkali-containing, glass. On the quartzites in the andesites from Strombolicchio there is noticeable around every quartzite grain a dark zone which turns out under high magnification to consist of crystalline microliths. Lacroix has described the same thing on a larger scale from other localities.

In some places, on the contrary, a change to tridymite has taken place. Thus Lacroix describes the marginal alteration of quartz to tridymite in certain inclusions in the leucite of the Eifel, from the Laacher Sea, etc. At Mayen (Eifel) Lacroix also detected cristobalite in the octahedrons surrounding a quartz grain (*l. c.*, p. 31). Nevertheless, with very few exceptions, *marginal* crystallizations alone have taken place. This clearly indicates the influence of liquid magmas and their vapors. The changes are *not to be considered as direct temperature transformations,* as such would necessarily penetrate into the interior as well. The study of inclusions seems to indicate that the region

[1] Macon, 1893.

of stability of quartz extends above 900°. The appearance of tridymite and cristobalite is to be ascribed less to the high temperature than to the quick crystallization in the presence of vapors.

Tridymite[1] and *chalcedonite* were found by W. J. Müller and the writer[2] to appear as original substances and as the product of rapid separation. In water solutions below 400°, this was in the absence of mineralizers (carbonates, etc.) *Quartz* was the stable product which appeared in the filter tube in our experiments. Thus, in the laboratory as well as in nature tridymite may crystallize below 800°. *Chalcedonite* is probably stable in water under 150°.

The transformation of quartz to tridymite thus gives no reliable temperature determination, neither does that of quartz to cristobalite. According to our experiments, the chemistry of the silicates, especially at lower temperatures, in part, resembles the organic chemistry of the carbon compounds. As with them, it is a chemistry of unstable compounds which frequently does not allow of description by the phase rule.

In connection with the tendency of rapid crystallization to result in the production of tridymite by preference, it is worth noting that the tridymite and cristobalite do not appear to occur pure, according to the analyses[3] submitted up to the present time. Perhaps the range of existence is greatly extended over that of stability by isomorphous impurities.

[1] The very little artificial tridymite obtained by us shows an index of refraction n_o for the *D*-line, which is about 1.43. This determination is according to the method of light lines of Becke. It therefore agrees much better with the measurement of Lasaulx (average 1.42) than with that of Mallard (1.48) on natural tridymite from the Auvergne. The tridymite from Cerro S. Cristobal, as I recently found, has $n_o = 1.48$.

[2] J. Koenigsberger and W. J. Müller, *Centralbl. f. Min., etc.*, 1906, pp. 339 and 353.

[3] See C. Hintze, "Handbuch d. Mineral," 2, 1461 and 1463.

IV. SIMULTANEOUS OCCURRENCES OF THE DIFFERENT SiO_2- AND TiO_2-MINERALS IN NATURE.

A noticeable physical-chemical paradox, if the phase rule is applicable here, is the common occurrence and intergrowth of different modifications of SiO_2 and also TiO_2. Tridymite and cristobalite, brookite and anatase, brookite, anatase and rutile, anatase and rutile, and still further quartz and tridymite, in the same rock are found together, even in rather regular intergrowths.

According to the fundamental laws of physical chemistry, only one solid form is stable within a given interval of temperature. Properly speaking, we should find either tridymite or cristobalite as the rock mineral, separate from quartz; the latter should form only at lower temperatures. In nature the reverse condition is found. Tridymite occurs where volcanic rocks already crystallizing were attacked by vapors (probably water). A. Lacroix proposes this for the tridymite in the andesite of Mont Pelée.[1] The vapor was probably there at the time of solidification, as most of the tridymite occurs in blow holes in the rock, and these, as is plainly seen, were not opened by secondary action. I should like to describe here briefly some occurrences which I have studied on the spot, in order to explain the condition in nature more clearly.

(*a*) *Strombolicchio at Stromboli* (*Sicily*).

A. Bergeat[2] has thoroughly investigated this locality petrographically and mineralogically. He has also published a bibliography of the earlier literature. The genetic relations alone will be presented here, as they have hitherto received much less attention. The small island in its western third consists of a strikingly red augite-andesite II. (Fig. 80). In the remaining portion it consists of a gray-green augite-andesite I. (Fig. 79). This is not in any way to be attributed to accidental weathering. Furthermore, blasting for the building of a signal station has shown that

[1] A. Lacroix, "La Montagne Pelée," p. 518 et seq., Paris, 1904.

[2] A. Bergeat, "Die äolischen Inseln," *Abh. k. bayr. Akad.*, 20, Abt. I., 1899, p. 49.

this distinction extends far below the surface. The red augite-andesite contains cavities lined with tridymite; the gray-green does not. Both contain numerous inclusions of quartzite and marl. In thin section, andesite I. (Fig. 79) shows a large amount of labradorite, diopside-augite, and very little olivine. These are enclosed in a ground mass of feldspar microliths, augite, biotite (?), and ores. In andesite II. (Fig. 80) the individuals of labradorite, and especially olivine, are more numerous. The ground mass is somewhat less prominent. Further-

FIG. 79. Augite-andesite I., fresh (Strombolicchio, Sicily), without tridymite druses. (Abut 50 times mag.)

FIG. 80. Porous, partially protematamorphic augite II., from Strombolicchio.

more, the formation of bands of iron oxyhydrates on the olivine, diopsides, and ores, is characteristic. Also characteristic is the development of very small, reddish yellow, strongly double refractive, rhombic (?) prisms. The augites of the ground mass are decomposed. The inclusions of quartzite which now comprise quartz grains are entirely unaltered in andesite I. and andesite II. The quartzes are surrounded by a dark zone which may be resolved by strong magnification into microliths. The double refraction of the quartz is unchanged even up to the very margins; neither tridymite nor cristobalite has been developed there (Fig. 81).

The mineral-lined cavities develop freely next to the quartzite, but only in andesite II. Evidently, the vapors gathered more

easily around the inclusion and the vapor pressure then kept the cavities open.

The vapors did not have very great pressure and density; for the superimposed mass could have had at most a thickness of 300–500 m. This corresponds to a pressure of 200 atmospheres, and consequently, at 900° it produces but a small fraction, about ⅓–⅕, of the critical density of water and also carbonic acid. This quantity of vapor, therefore, could exert little or no solvent action, and so be able to decompose only the olivine and augite.

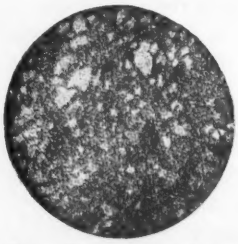


FIG. 81. Quartz inclusion. The quartz grains surrounded by dark zone of microliths, but without tridymite formations. In augite-andesite (Strombolicchio). (× ca. 50 diam.)

but not the plagioclase.[1] The pores are therefore empty. The hornblendes, hypersthenes, etc., of the sides are clearly formed from the molten mass; the small tridymite crystals alone are the products of solution. The entire rock is full of little pores, like the porous Nordmarkite at the Tonsenplads at Christiania. In these little cavities mineral druses are found, and among the minerals the tridymite is abundantly present. The tridymite is not, however, an original rock-making mineral.

From the standpoint of paragenesis, the sequence is in agreement with Bergeat: 1, hornblende; on this 2, anorthite, also some clear green fassaite-augite, hypersthene and simultaneously as well as later 2, and 3, tridymite. The presence of steam here as

[1] In the Alpine mineral veins, in which the concentration of the steam was almost equal to the critical density, the plagioclase in the vicinity of the fissure is gone.

elsewhere favored the formation of hornblende instead of augite. In the writer's opinion, there is not a sharp temperature limit between augite and hornblende. Why a part of the rock only was saturated with steam and the rest was not, cannot be stated. I would prefer, however, to assume that secondary circumstances (nearness to the sea, accumulation of water as on Mont Pelée, etc.) were the cause, rather than the presence of juvenile water.

(*b*) *Cerro San Cristobal.*

The mineral locality in the gorge about 30 km. southeast from the summit of Cerro S. Cristobal in Pachuca (Mexico, state of Hidalgo), is famous for the discovery of the two forms of silicic acid, tridymite, and cristobalite. These were discovered by G. vom Rath.[1] The crystallographic properties of the minerals from Cerro S. Cristobal have been made known through the investigations of G. vom Rath. The optical behavior was studied by M. Bauer, v. Lasaulx, and particularly by Mallard.

According to Mallard, tridymite first acquires its crystallographic and optical symmetry at 130°; cristobalite at 175°. This proves that both were formed above this temperature.

In this paper the paragenesis only will be discussed, because this has been only very briefly considered by G. vom Rath.[2]

The minerals are found in the cavities (Fig. 82) of a rather fine-grained augite-andesite which is divided, like that of Strombolicchio, into a red porous portion and a dark green non-porous one. The two are not sharply separated. Diopside-augite, basic plagioclase, also sanidine form small individuals in the fresh rock. The red ground mass consists of augite, plagioclase, magnetite, orthoclase, augite (hornblende?). The porous rock is similarly composed, only it shows much decomposition. The diopside individuals and the magnetite grains are surrounded by a red border rich in iron oxides. The augite of the ground mass is permeated through with flakes of Fe_2O_3. The very small yellow-red prisms

[1] Literature by C. Hintze, "Handbuch d. Mineral," II., p. 1446 *et seq.*

[2] G. vom Rath, *Correspondenzblatt Naturhist. Vereins Rheinlande*, 43, 1886, p. 114.

mentioned at Strombolicchio are also found here in the porous rock. The plagioclase has remained unaltered; hence, here also the amount of steam was small. G. vom Rath[1] has given an analysis of this rock. The rock is filled with large and small, long-drawn-out or round pores, as can be seen in Fig. 82.

FIG. 82. Numerous tridymite druses in the andesite of Cerro S. Cristobal. (About 1/10 natural size.)

According to my experience, a close connection does not exist between the quartz inclusions, which are, however, very scarce, and the tridymite druses. Most of the pores contain magnetite, less often, hornblende and tridymite; only in the upper part of the gorge, nearly under the summit, on the curve of a little path, does cristobalite appear, accompanied by magnetite and specu-

[1] G. vom Rath, *Pogg. Ann.*, 135, 446 (1888).

larite. Tridymite and cristobalite are associated, but do not regularly grow together. In most of the pores, both minerals are white and non-transparent, but in quite a number of them some of the tridymite crystals are glass clear. The cristobalite crystals are rarely so. The rock is permeated besides with numerous small holes completely filled with tridymite and cristobalite.

The dark-gray fresh andesite, which contains no tridymite, was later subjected to pneumatolytic action. Many small crevices were developed in it which were partially filled with quartz and

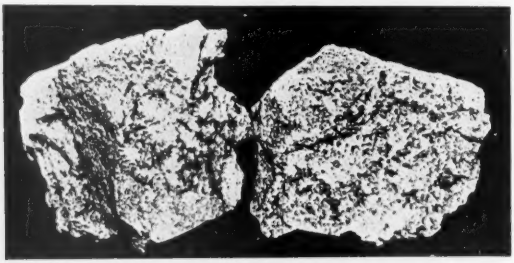


FIG. 83. Tridymite-cristobalite druses, Hypersthene-tridymite druses, Cerro S. Cristobal. Strombolicchio.

manganese iron oxides. These cavities are very probably connected with the famous metallic veins of Pachuca, as, for example, Porvenir, which outcrop from above through Cerro S. Cristobal. As to the metallic veins, no difference exists between the underlying quartz porphyries, or, to speak more correctly, the trachytes, and the overlying andesites. Whether petrographically a sharp distinction exists between the acid and basic rocks, I cannot say. Only this is certain: the cavities with quartz and metals are secondary, while the tridymite pores are primary.

It can be said in general that in most cases tridymite, and not a rock-making mineral, crystallizes in the cavities. This is true not only at Strombolicchio and Cerro S. Cristobal, but elsewhere. Tridymite in nature has been formed mostly at temperatures somewhat lower than its melting point, soon after the solidification of the rock and under the action of vapors.

(*c*) *Titanium Dioxide Localities of the Eastern Aar Masses.*

In the fissure veins which occur in certain rocks, the sericite schists and mica schists of the Aar and Gotthard masses, and in general of the Alps, titanium dioxide minerals, usually in several modifications, are regularly found as associated minerals. In another place,[1] the paragenesis and the locality of occurrence in the Aar mass are thoroughly described. The formation of the minerals proceeded in Tertiary time from water solution by leaching of the rock, together with dynamic metamorphism. This occurred at temperatures between 250° and 400°. The occurrences are typical for Alpine deposits and are characterized by a quartz band as is well shown in Fig. 84.

The simultaneous occurrence of brookite and anatase is most frequent. The pyramids of the latter rest on the plates of brookite and penetrate them. Anatase is more frequent in the true mica schists, brookite in the sericite gneisses. In the sericite schists both occur simultaneously and also with rutile. Rutile occurs mostly in fine dark-brown needles which penetrate the anatase or around which anatase crystals have formed. The three minerals have crystallized thus simultaneously in several localities in the upper Griesernal (Terrasse), Maderanertal (Canton Uri), in the Rieder Tobel on the Reuss (Canton Uri at 'Amsteg).

Anatase and rutile without brookite seldom occur together and then rutile forms mostly pseudomorphs after anatase, as at Culm da Vi at Sedrun, in crevices in aplitic-dioritic fissures. Brookite and rutile together without anatase are lacking in the Aar and Gotthard masses, and, indeed, also elsewhere. Each of the three minerals occurs alone frequently. Anatase in certain mica schists rich in mica, rutile in veins in sericite phyllite rich in iron ochre, brookite in the sericite gneisses carying feldspar. The titanium dioxides are younger than the albite and adular. They are simultaneous with, or only slightly younger than quartz. Their varying occurrence is evidently not due to temperature limits.

[1] J. Koenigsberger, "Geol. u. Mineral. Karte des östl. Aarmassivs nebst Erläuterungen." Freiburg i. B., 1910, p. 50.

FIG. 84. Typical Alpine mineral deposits in sericite gneiss-sericite schist. Anatase, more rarely rutile and brookite with quartz, adular, albite. On glacier under the Hagstöcken (Düssistock, Maderanertal, Aarmassiv, Switzerland).

Possibly it is the result of other chemical compounds, perhaps such as isomorphous impurities in the solution, thus rutile to iron carbonate and oxide, and much quartz, anatase to magnesium silicate.

V. CHEMICAL REACTIONS AS GEOLOGICAL THERMOMETERS.

(*a*) *The Dissociation of Calcium Carbonate as a Thermometer.*

It is known that at high temperatures, calcium carbonate yields carbonic acid, provided the external pressure of carbonic acid is not greater than the dissociation tension of calcium carbonate. The first measurement of the pressure of carbonic acid over calcium carbonate at a definite temperature was made by Le Chatelier.[1] E. H. Riesenfeld[2] with E. Pott and C. A. Cohn, Zavrieff[3] and J. Johnston[4] have made the most recent exact measurements. The measurements of these authors agree well with each other, but there are disagreements in respect to the extrapolation formula. The formula of E. H. Riesenfeld is in harmony with the theorem of W. Nernst. The latter, undoubtedly, gives in part a fair approximation of the truth. It is useful through a greater range of measurement (1,600 mm.) than that of Johnston (716 mm.). We present them therefore hypothetically with the following observations. The pressures obtained from them are as follows:

Temp. Centigrade.	Atmosph. Pressure.	Meters of Rock, $s = 2.5$.
910	1	4
1,000	4	16
1,100	20	80
1,200	170	680
1,300	2,600	10,400
1,400	80,000	320,000

Neither the purity nor the crystal habit (aragonite, calcite) has any appreciable influence according to the experiments of Riesenfeld.

[1] Le Chatelier, *Compt. rend.*, 102, 1243 (1886).
[2] E. H. Riesenfeld, *Jour. chim. phys.*, 7, 561 (1909).
[3] Zavrieff, *Jour. chim. phys.*, 7, 31 (1909).
[4] J. Johnston, *Jour. Amer. Chem. Soc.*, 32, 938 (1910).

The pressures, or, in other words, the meters of overlying rock, give us directly the maximum temperature. This is true because: *there is no possible way in which any appreciable quantities of pure limestone can fail to decompose.* A retardation such as can enter into transformations is not possible. For these reasons, if any calcium carbonate at all is present, its temperature must always have been lower than that which is required by the pressure of the superimposed rock mass. The pressure p in atmospheres which the rock mass exerts, if it concerns rather large surfaces, is in reality given by the thickness of the overlying rock ($p = h.s/10$). The same thing holds true if a magma is under consideration. The viscosity of the lava is unimportant, and inasmuch as its effect is but momentary, involves a correction of only 10–50 atmospheres. In the case of explosive decomposition, entirely other factors enter the problem. *When undecomposed calcium carbonate forms inclusions on the borders of a magma, it is therefore a reliable maximum thermometer. On the other hand, the decomposition of calcium carbonate, if calcium silicate be formed, does not furnish a reliable end point.* J. Müller[1] and the writer have shown that in water solutions silicic acid is stronger than carbonic acid at as low a temperature even as 260°. Hence, garnet, epidote, and vesuvianite can be formed by hydato-pneumatolytic contact metamorphism below the decomposition point of calcium carbonate. It is here immaterial whether the water carries with it aluminum silicate solutions or causes a reaction between the aluminum silicate and the calcium carbonate of impure limestones. The same holds true for the contact of limestone with a liquid magma. For chemical reasons, then, calcium carbonate will in general seldom occur as an inclusion in volcanic rocks.

Calcium carbonate can exist in a magma only when the two following conditions are simultaneously met. (1) The magma must be saturated with CaO. (2) The temperature of the magma must require a smaller dissociation tension than corre-

[1] J. Müller and J. Koenigsberger, *Centralbl. f. Min., etc.*, 1906, pp. 339 and 353.

sponds to the existing pressure. Such a magma is evidently the nepheline syenite of Alnö studied by A. G. Högbom.[1] The limestone inclusions (silurian?) were melted without being destroyed. Hence, calcium carbonate occurs there in graphic granitic intergrowth with olivine and aegirine.[1] The simultaneous pyrogenetic crystallization is not to be doubted.

In order to determine the temperature, an approximate estimate of the pressure is necessary. A layer more than 50 km. thick is highly improbable, and so the liquid magma, according to the above table, must have possessed a temperature lower than 1300°. Again, marble inclusions from the phonolite of Oberschaffhausen are further described by F. Graeff.[2] There the solidification occurred not very far below the surface, as the whole geology of the Kaiserstuhl shows that the now buried rocks did not solidify at a greater depth than 500–800 m. A maximum temperature for the liquid magma is consequently 1100–1200°. Numerous other examples are given in the work of A. Lacroix.

Places in which larger lava streams have flowed over limestone strata give us wider limiting points. I have seen such a flow at Kula Dewlit in Katakekaumene. The kulaite (hypersthene basalt) covers Tertiary lacustrian limestone, and also calcareous mica schists of much older periods. In none has it produced a noticeable change on the border. The lava flow which is 10–30 m. thick, 1–5 km. broad, and 10 km. long, must have heated the underlying limestone strata at their surfaces to practically its mean temperature (also by radiation, notwithstanding the possible Leydenfrost phenomenon). If the pressure of the carbonic acid were greater than the pressure of the superimposed lava, it would have burst it open. In the lower part of the flow, where the thickness is about 10 m., this was not the case. From the table its temperature must, therefore, have been below 1000° At the upper end of the flow, where it has been poured out through a side fissure on to Kula Dewlit, there are peculiar little

[1] A. G. Högbom, *Sverig. Geol. Undersökn. C.*, No. 148, Stockholm, 1895, and *Guide Congr. géol. intern.*, Stockholm, No. 3. Rachel Workman, *Geol. Mag.*, 8, 193 (1911).

[2] F. Graeff, *Mitteil. bad. geol. Landesanst.*, 14, 462 (1892).

peaks, lava craters. Many so-called "Sommabröcken," lime silicates which were probably thrown out from the crater, are found at the summit of Kula Dewlit. Perhaps the temperature of the upper end of the flow was therefore above 1100°.

I should like to enter here briefly into an old discussion, whether limestones are altered by contact metamorphism into lime silicates. As is well known, one side contends that lime silicates, or lime aluminum silicates, can be formed only when silicious or argillaceous lime was originally present. The other side holds that the ingredients were introduced from the magma. Geological and chemical proofs have been advanced in support of both sides. In my opinion, the physical-chemical solution of this controversy is as follows:

If the calcium carbonate remains undissociated, or if little or no water enters as a solution or vapor, no appreciable change will take place. There will be only a recrystallization to marble. The new forms in the interior then depend only on the primary impurities.

If, however, the dissociation pressure of the carbonic acid gas is really higher than corresponds to the weight of the overlying rock (thus, at high temperatures of magmas the formation of crystalline schists), then it escapes and the remaining CaO will unite with the liquid magma to form lime silicates. The higher the temperature, the more does the affinity of silicic acid exceed that of carbonic acid. Water solutions or vapors containing silicic acid or alumina work analogously. Hence, in pneumatolytic contact metamorphism copious lime silicates appear directly where the vapors circulate. In the interior of the larger lime masses, where the vapors do not penetrate, they are lacking (except in fissures), or are present only so far as they correspond to the original composition of the rock.

Only where CaOH or CaO forms the inclusion does the objection to the use of chemical reaction for temperature determinations disappear. Several such cases have been described by A. Lacroix. Generally the change to lime, hydrogiobertite, hydrodolomite, etc., takes place under the effects of the atmosphere,

although that is of secondary importance for the explanation. Such inclusions, which were originally CaO, have been observed by Lacroix on Somma and Eifel. They show that the magma when it enclosed the lime pebbles must have been above 1100°.

The occurrences of marble and lime silicates in the crystalline schists are worthy of note. The former belongs really to dynamic metamorphic change, and the latter to contact metamorphic change.

In pegmatites, whose solidification temperature is presumably lower than that of Plutonic and effusive rocks, L. Duparc[1] has found pebbles of cepollino-marble. It is noticeable that lime inclusions are very rare in deep rocks. In the types between volcanic and deep rocks, as, for example, in monzonite from Predazzo, marbles are preserved as fragments. In the same way, pebbles of limestone or marble are found directly at the margins of magmas. Here the magmas were already somewhat chilled by the intrusion. The fragments are not torn from a deep-seated source, but from the immediately adjacent rock. W. Salomon[2] has observed instances of this kind in the tonalite of Adamello, which borders on triassic limestone. The formation of marble indicates the crystallization of lime in CO_2. If we assume the overlying rock to have a thickness of 1,500 m., a temperature below 1,230° is required. This is small for a Plutonic rock. Therefore, the intensity of contact metamorphism at Adamello is not very great. An actual mixing and melting has not occurred there, according to the investigations of Salomon. That no formations of silicates took place on the margins indicates the absence of pneumatolytic-hydatogenetic effects and of metalliferous deposits both at the time of and after the intrusion of the tonalite. These are in truth lacking there.

(*b*) *Other Chemical Reactions.*

Other reversible chemical reactions have been employed to a small extent only for temperature determinations. In some very

[1] L. Duparc, "Mineraux des pegmatites de Madagascar," p. 322, Geneva.

[2] W. Salomon, "Adamellogruppe," *Abh. k. k. geol. Reichsanst.*, Vienna, 1910, 2, 489.

interesting experiments, A. Brun[1] ascribes a limiting point for the solidification temperature of those acid lavas from which quartz will separate under certain circumstances. He found that obsidian from Lipari and other places explodes in a vacuum or at atmospheric pressure between 900° and 1200°; that it gives off gases which are chiefly chlorine and chlorine compounds (Cl_2, HCl, NH_4Cl) besides CO_2, CO, SO_2. Brun correctly infers that obsidian which has not been chilled at too great depths has reached the surface cooler than 900°. At greater depths, where the pressure hindered the escape of the gases, the temperature could have been actually higher. Brun's conclusions concerning the co-volume do not appear to me convincing. A flask of water containing carbonic acid will also "explode" if it becomes too warm. The relation of pressure to temperature for absorbed gases cannot generally be determined.

The reaction $2CO_2 = 2CO + O_2$ might render a temperature determination possible. From the analyses of fumarole gases by Brun (he found the ratio $CO : CO_2$ to be between 0.14 and 0.45), the conclusion is drawn that the initial temperature of the lava was very much higher,—over 3000°. This necessitates the assumption that the quotient $CO : CO_2$ results from the dissociation and not from the quantity of oxygen which is accidentally present. If this assumption be correct, the oxygen must have occurred in an amount of more than ¼ of the percentage of CO. —the gases studied by Brun; this happens, however, in only a few cases.[2] The conclusion is, therefore, very uncertain.

There is, however, no objection to the conclusion of Brun that the obsidian solidified on the surface below its "explosive temperature," 950–1000°. On the other hand, the occurrence of obsidian pumice (as at Lipari), and of woolly andesite (as at Stromboli) shows that the lavas were heated at the surface far above the explosive temperature, that, therefore, even there the temperature has been to this limit. The dissociation of water,

[1] A. Brun, *Archiv. Scien. phys. nat. Genève*, 14, May and June, 1905; 17, Feb. and July, 1909. *Neu. Jahrb. f. Min., etc.*, Vol. 32.

[2] A. Brun, *Arch. Sc.*, 17, July, 1909.

which could serve also for temperature determinations, is not applicable. We know too little of the source of the hydrogen and oxygen in fumaroles.

VI. DIRECT MEASUREMENTS OF TEMPERATURE OF SOLIDIFYING MAGMAS.

Up to the present time, only a few direct temperature measurements of magmas have been made.

C. Doelter[1] made an observation at Vesuvius in which the temperature of the lava flow was 1000–1070°.

Silvestri observed that a silver wire did not melt when introduced into lava from Etna. This experiment is indefinite because of the high conductivity of silver wire; otherwise, it would indicate a temperature below 960°. Bartoli found 1063° in 1902 in the flowing lavas of Etna. On the other hand, the radiation of the lava has melted the copper vessels found in houses. This would indicate a temperature above 1084°. The observations are not numerous, and it is difficult to tell definitely how the sources of error may be eliminated. The melting experiments with thick wires are uncertain on account of the conduction of heat. The introduction of a thermoelement of platinum-platinum rhodium to a depth of 50 cm. in the lava would be the best plan. The poor conductivity of the lava must be considered. On the surface the flowing lava is a half solidified mixture, or in reality, a solid membrane broken to pieces, while it remains much hotter at a slight depth. The temperature must be taken the moment the thermoelement is held fast by the solidified lava.

The radiations which are emitted also furnish us with further limits. The emission of light by lavas corresponds to that of a dark body, on account of their small refractive power (5 % of the radiation falling on them) and their relatively strong absorption. The temperature can easily be ascertained, therefore, at a safe distance of 10 m. or more, by making use of the pyrometer. This may be done, according to Le Chatelier, by comparison with a lamp, or by means of Wanner's pyrometer. The temperature

[1] C. Doelter, "Petrogenesis," p. 16, Braunschweig, 1906.

of the surface only (1–10 cm. deep) is obtained in this way, owing to the strong absorption of light by the lava. The results are uncertain, and do not correspond to the temperature of solidification; they are too low.

While the light given out by flowing lava is a strong red (850–950°), strong yellow (over 1000°) is seen as the lava first pours out. For example, B. H. Piutti and G. Magli observed this in the lava from Etna in 1909.

When I was in the Boccha piccola of Stromboli, in 1905, the lava shone bright red by day, corresponding to about 950–1000°. The vapors of SO_2, HCl, and NH_4Cl were condensed in the air, partially, indeed, by water vapor, and these clouds hindered the heat determination. In general, the temperature of these lavas, as long as they flow, is hardly under 950°. Direct temperature measurements are not obtainable for acid volcanic rocks, especially those which separate quartz.

CONCLUSIONS.

The dissociation of calcium carbonate indicates from its occurrence in nature the following:

1. Some magmas (as Eifel) possessed at their extrusion temperatures which were certainly above 1000°. Quartz, nevertheless, has been contained in them as inclusions.

2. Some magmas (as the Kaiserstuhl) were, on the other hand, already cooler than 1100–1200° at greater depths. Their solidification period must, therefore, have been below 1100°.

3. In Plutonic rocks, calcium carbonate is rarely found. It then occurs either when the liquid magma was saturated with CaO and the temperature as required by the pressure was below the dissociation temperature (Alnö), or when the magma was nearly solid and its contact effects were free from pneumatolytic influences (Adamello).

The polymorphic forms of SiO_2 show the following relations:

4. The change of α- into β-quartz is very probably a reliable thermometer.

5. The change of quartz into tridymite or cristobalite does not, however, give a definite temperature determination. Quartz is

capable of existence certainly far over 800°, perhaps to 1050°, as its occurrence in the inclusions of volcanic rocks shows where tridymite occurs in the pores.

On the other hand, tridymite and cristobalite crystallize even in nature at temperatures lower than 900°, and laboratory experiments with water solutions at 350° confirm this with regard to tridymite. To the present time, there is no ground, according to my experience, to assume the solidification temperature to be lower than 1000°.

We could assign provisionally as the upper limit, only the melting temperature of basic magmas, 1150°. How far the field of stability of quartz extends is not to be determined with certainty as yet by laboratory experiments. This is true because by quick solidification and small pressure tridymite is formed even below its field of stability. Hence, the field of stability of quartz appears to be forced too far down.

6. The observations of mineral localities show:

Several forms of *one* substance are sometimes simultaneously crystallized from water solutions, as tridymite and cristobalite, rutile, anatase and brookite, etc. In their association or occurrence and stability, there exists an analogy between quartz and rutile, tridymite and brookite, anatase and cristobalite. For the occurrence in nature, the field of existence[1] of a modification, and its preference appear to be caused much more by other physical and chemical circumstances (pressure, quickness of crystallization, isomorphous impurities, mineralizers) than by the field of stability. The greater the difference in physical and chemical contact of two modifications, the more the field of existence extends over the field of stability, and the greater is the influence perhaps, which small isomorphous impurities and other things exert upon the field of existence. The smaller the difference in physical and chemical contact of two modifications, the more exactly the fields of existence and stability coincide, and the more suitable is the transition point for geological temperature determination.

[1] We leave undetermined the question whether the field of existence is coextensive with the field of stability, or extends beyond it.

DISCUSSION

This department has been established by the editors in order to afford to those interested in questions relating to economic geology an opportunity for informal discussion. Contributions are cordially invited either in the form of discussion of more formal papers appearing in earlier numbers or bearing upon matters not previously treated. Letters should be directed to the Editor, Sheffield Scientific School of Yale University, New Haven, Conn. The full name of the author should be attached to all communications.

THE NECESSITY FOR A THEORY OF DIFFERENTIAL CEMENTING IN PROSPECTING FOR OIL.

In prospecting for oil and gas, a knowledge of the attitude of the oil-bearing reservoir or sand body and of the horizon is no longer adequate. The shape of the reservoir must also be made out as nearly as possible. The analogy of nearby sand bodies and of sand bodies as we see them in the making is useful. But we have, in addition, a disturbing factor. As an example, we may take the Hartshorne sandstone of Oklahoma and Arkansas. The sand grains are nearly all siliceous and the sizes are unusually uniform throughout the length and thickness of this great mass, which sometimes exceeds three hundred feet in thickness and which is one hundred and eighty miles in length along its outcrop. Yet this sandstone is gas-bearing or water-bearing only in pay streaks making up a small fraction of the total thickness. These streaks seem to differ only in the quantity of cementing material, which is partly gypsum. Their linear extent is limited. Since the size and nature of the sand grains are favorable throughout, we have then an additional factor in oil and gas accumulation—differential cementing. I believe we have frequently attributed to sand texture conditions which are really the result of the cementing.

Sounder oil and gas prospecting must therefore await, in addi-

tion for other aids, a better knowledge of the data and causes of differential cementing in sandstone.

ROSWELL H. JOHNSON.

REPLACEMENT OF SILICIOUS ROCK BY PYRITE.

In the April–May, 1912, issue of ECONOMIC GEOLOGY, Mr. H. H. Knox calls attention to the frequent replacement of silicious rock by pyrite. He mentions among other examples certain pyritic lenses in the Urals, where the pyrite replaces talc and chlorite schists. In 1911 I had occasion to examine these lenses, a brief description of which may be found in the *Mining Magazine* for June, 1912. The microscope shows that while these schists contain much talc and chlorite, and sometimes calcite, they are chiefly made up of secondary granular quartz. I can corroborate the observations of Knox as to the replacement of the schist by pyrite, every gradation being found from schist sprinkled with pyrite to solid pyrite. Moreover, the preference of the pyrite for schist is strikingly shown by the cores of a deep bore under the Kyshtim iron mine. Here there are several layers of schist from a few inches to 25 feet in thickness interbedded with much thicker layers of marble. The schist layers are all impregnated or at least sprinkled with pyrite, while the marble contains only traces or none at all.

In fact it is probably true that most pyritic lenses of the Rio Tinto type were formed by the replacement of *silicate* minerals and not of *carbonates*. This is quite certainly true of all the pyritic lenses now being mined in the Urals and of the lenses of the Mountain Copper Mine in California, where the chief country rock is alaskite-porphyry, and where the original porphyritic quartzes of the porphyry are sometimes to be noted in the ore itself. It is pertinent to note that the Rio Tinto and other pyrite lenses of the Spain-Portugal field, that of the United Verde in Arizona, and the Lyell lenses in Tasmania are all in relatively silicious rocks, mostly schists.

H. W. TURNER.

REVIEWS

Pillow-Lava from the Kiruna District. By N. SUNDIUS. Reprint from Geologiska Föreningens I Stockholm Förhandlingar, Mars, 1912.

There has been diversity of opinion among geologists as to the conditions of development of pillow-lavas. Most of the students of the subject in recent years seem to agree that they must be a submarine phenomenon, and the author, in his description of the pillow-lavas of the Kiruna district, corroborates their views.

From his microscopic studies he finds that the greenstone pillows originally consisted of an outer glassy crust 2–3 mm. thick, passing into a hypocrystalline interior, sometimes with fluidal structures developed near the surface and running parallel to the latter, and with a vesicular zone next to the glassy crust. From the evidence found in the field and under the microscope, the author believes that the occurrences of the pillows must be considered as a flow-phenomenon in the lavas, where each pillow has formed an individual with its own surface of cooling.

The association of the pillow structure in the Kiruna district with known shallow water deposited conglomerate, as well as their known association with other marine deposits as radiolarian cherts, slates, and limestones in other areas, leads the author to conclude that contact with water has been the factor that has favored the development of the pillow structure. This view is strengthened by the observation of Tempest Anderson, who has seen the pillows form at Savaii, as the lava came in contact with the water.

ISIDOR L. REHFUSS.

RECENT LITERATURE ON ECONOMIC GEOLOGY.

COMPILED BY

JOSEPH B. UMPLEBY.

METALLIFEROUS DEPOSITS.

COPPER.

Geology and Ore Deposits of the Butte District, Montana. By W. H. WEED. Prof. Paper U. S. Geol. Survey No. 74, 1912, pp. 262, plates 41, figs. 109.

This paper represents the first extensive study of the Butte district —a district which produced, up to the close of 1906, approximately $650,000,000 and which during recent years has had an annual production exceeded only by the Rand, in South Africa.

LEAD-SILVER.

Geology and Ore Deposits of the Park City District, Utah. By. J. M. BOUTWELL. Prof. Paper U. S. Geol. Survey No. 77, pp. 231, plates 44, figs. 18.

Park City, Utah, throughout most of the period since its discovery in 1869, has ranked among the more important lead producing camps of the world. The ores of the Park City district occur as lode deposits and as bedded deposits enclosed in sedimentary and intrusive rocks.

GOLD AND SILVER.

Notes on Mining in Seward Peninsula, Alaska. By PHILIP S. SMITH. U. S. Geol. Survey, Bull. 520-M, Washington, 1912, pp. 8.

Seward Peninsula is still of prime importance because of its gold placers although recently interest has been renewed in the deposits of placer tin on Buck Creek, and a production of $50,000 is reported from them for the past year. Tin also occurs in lodes near York.

Gold, Silver, Copper, Lead, and Zinc in the Eastern States in 1911. By H. D. McCaskey. Adv. Ch. Min. Res. U. S., Washington, 1912, pp. 18.

The total value of the mine output in the Eastern States of gold, silver, copper, lead, and zinc for the calendar year 1911 was $11,787,942, representing the production of 96 mines, of which 57 were gold placers.

Notes on the Antelope District, Nevada. By F. C. Schrader. U. S. Geol. Survey, Bull. 530-J, Washington, 1912, pp. 14.

The metalliferous deposits of the district are silver and gold veins enclosed in Tertiary volcanic rocks.

Gold and Silver in Idaho. By J. B. Umpleby, F. C. Calkins, and E. L. Jones, Jr. U. S. Geol. Survey, Bull. 530-G, Washington, 1912, pp. 23.

The Loon Creek district is of special interest because of its gold-copper bearing veins. Two to three ounces of gold occur in a chalcopyrite ore containing about 10 per cent. copper.

The St. Joe-Clearwater Region contains three types of gold-silver bearing veins: (1) Quartz veins containing a little feldspar and mica. (2) Quartz veins containing much chlorite and more or less calcite. And (3) Veins composed of carbonates and quartz in nearly equal quantity.

Notes on the Gold Lodes of the Carrville District, Trinity County, Calif. By D. F. MacDonald. U. S. Geol. Survey, Bull. 530-D, Washington, 1912, pp. 37.

Describes briefly the five types of gold lodes here found and is accompanied by a sketch map of the areal geology.

Geology and Mineral Resources of the Chibougaman Region, Quebec. By the Chibougaman Mining Commission. Province of Quebec Dept. of Colonization, Mines and Fisheries, Quebec, 1911, pp. 215, plates 78, figs. 19.

The Chibougaman region contains widely disseminated deposits of gold, silver, copper, nickel, and asbestos which, however, have not been shown to occur in bodies large enough to warrant the Commission in recommending the building of a railroad.

TUNGSTEN.

The Tungsten Mining Industry in New South Wales, 1911. By J. E. Carne. Sydney, 1912, pp. 98.

The object of this work as stated by the author is "to collect in handy form items of special interest from published reports and articles, and to give a connected account of the metal, its ores, mining, concentration, metallurgy and uses."

STATISTICAL REPORTS.

Quicksilver in 1911. By H. D. McCaskey. Adv. Ch. Min. Res. 1911 U. S., Washington, 1912, pp. 35.

During 1911 the output of quicksilver in the United States was 21,256 flasks of 75 pounds each, of which 18,860 flasks were produced in California and the remainder in Nevada and Texas combined.

The Production of Abrasive Materials in 1911. By W. C. Phalen. Adv. Ch. Min. Res. U. S., 1911, Washington, 1912, pp. 22.

The year 1911 marked a decided increase over 1910 in the value of natural abrasives produced in the United States and a somewhat less decrease in the value of artificial abrasives. The total domestic production was valued at $3,019,803.

The Mineral Industry of New Jersey for 1911. By Henry B. Kümmel. New Jersey Geol. Survey, Bull. No. 7, 37 pp., Trenton, 1912.

The Production of Asphalt, Related Bitumens, and Bituminous Rock in 1911. By David T. Day. Adv. Ch. Min. Res. U. S., Washington, 1912, pp. 21.

The production of asphalt in the United States has increased with the movement for better roads. The general market condition is such that it is thought probable that, during the current year, additional sources of supply will be developed.

Statistics of the Pottery Industry in the United States in 1911. By Jefferson Middleton. Adv. Ch. Min. Res. U. S., Washington, 1912, pp. 11.

The year 1911 showed the greatest value of domestic pottery yet recorded, the increase in value coming from the lower grades of ware, and china, sanitary ware and porcelain electrical supplies.

The Gypsum Industry in 1911. By Ernest F. Burchard. Adv. Ch. Min. Res. U. S., Washington, 1912, pp. 8.

Noteworthy in the 1911 gypsum industry was the decreased output of gypsum products, the decreased average price of these products, the replacement of a number of old mills by a smaller number of larger modern mills, and the trend of the industry toward the manufacture of the finished products in commercial centers remote from gypsum deposits.

NON-METALLIFEROUS.

COAL.

Coal Fields in Montana. By L. J. Pepperberg and W. R. Calvert. U. S. Geol. Survey, Bull. 471-E, Washington, 1912, pp. 66.

Describes the stratigraphy and coal beds of the southern extension of the Milk River coal field, Chouteau County; the Livingston and Trail Creek coal fields, Park, Gallatin, and Sweetgrass counties; and the Electric coal field, Park County, Montana.

Coals of the Region Drained by the Quicksand Creeks, Kentucky. By F. JULIUS FOHS. Kentucky Geol. Survey, Bull. No. 18, 79 pp. Louisville, 1912.

This paper describes the occurrence of the coal beds and records numerous detailed sections and analyses.

REGIONAL.

The Economic Geology of the Hartford Quadrangle, Kentucky. By JAMES H. GARDNER and S. C. JONES. Ky. Geol. Survey, 33 pp. Louisville, 1912.

Coals, iron ores, clay deposits, cement materials, oil, and gas, occur in this quadrangle. A study was also made of the soils of the area.

SCIENTIFIC NOTES AND NEWS[1]

INTERNATIONAL GEOLOGICAL WORK.—Dr. D. D. Cairnes, of the Geological Survey of Canada, who returned to Ottawa late in September, reports having completed that portion of the International geological work along the 141st meridian (the Yukon-Alaska boundary) which had been undertaken by the Canadian government, the United States and Canadian Geological Surveys having agreed to map the geology along the boundary line from the Yukon river to the Arctic ocean, a distance of about 350 miles. This work was commenced in the spring of 1911, and by the terms of the agreement, the United States and Canadian geologists worked to the north and south, respectively, of the Porcupine river, each extending their investigations at least two miles east and west of the boundary line. This work not only gives a geological section at that longitude through the northern half of the Yukon plateau, the entire Rocky Mountain, and the Arctic Slope physiographic provinces, but should also assist materially in correlating the geology of Alaska with that of Yukon Territory and British Columbia.

MR. JAMES H. GARDNER, formerly assistant geologist on the U. S. Geological Survey, who has recently been engaged in work on the clays, coals and oil fields for the State Geological Survey of Kentucky, has been engaged by the Topographic and Geologic Survey of Pennsylvania and given charge of the mapping and general study of the Broad Top Coalfield, with field headquarters at Hopewell, Pa.

DR. ROLLIN T. CHAMBERLIN, of the department of geology in the University of Chicago, has returned from a year of special

[1] Geologists, mining engineers and others interested in applied geology are invited to keep the editor informed of new investigations of mining districts or scientific studies undertaken by them, together with such other scientific and personal items as may come to their notice.

investigations in South America, where he went as a geologist of the Brazilian Iron and Steel Company to examine the recently recognized iron ore deposits in the state of Minas Geraes, famous in the past for its output of gold and diamonds but likely in the future to be best known for its unrivaled mountains of iron ore. Dr. Chamberlin's special work was to locate the most promising ore masses in the district, make geologic and topographic surveys, and estimate the quantity and value of the ore. The surveys were much hindered by the necessity of cutting trails through the tropical jungle. Natives armed with the Brazilian foica or wood hook were employed for the purpose. Travel was largely by mule-back. In order to get a general view of the geology of the South American continent Mr. Chamberlin, after finishing his work in Minas Geraes, traveled southward through Brazil and Uruguay to Buenos Aires and returned to the United States by way of the Straits of Magellan, Chile, Bolivia, Peru and Panama.

AMONG promotions in the faculty of Oberlin College is that of Dr. George David Hubbard, to be professor of geology on permanent appointment and head of the department.

THE COLORADO Geological Survey has had three parties in the field during the summer of 1912. One party, under the direction of Assistant Professor Ralph D. Crawford and Mr. Philip G. Worcester, has continued the work begun last season in the Gold-brick district. A second party, under the direction of Professor H. B. Patton, of the Colorado School of Mines, has spent the summer in the Bonanza Mining district near Villa Grove. A third party, directed by Dr. George I. Finlay, of Colorado College, has been at work on an investigation of the water resources of the Arkansas Valley region.

MR. J. W. MERRITT, assistant in mineralogy at Northwestern University, has been appointed instructor in geology at Dartmouth College.

DR. THOMAS CLACHAR BROWN, a graduate of Amherst, who received his doctorate from Columbia in 1909 and who has since filled the positions of assistant professor of geology at Middle-

bury College and assistant professor of geology at the School of Mines, Pennsylvania State College, has been appointed associate in geology at Bryn Mawr College.

P. S. SMITH arrived in Washington, D. C., on October 26, from a trip to Fairbanks, Alaska, where he investigated the present status of quartz lode mining in that vicinity.

L. C. BURLING, of the Smithsonian Institution, has been appointed paleontologist of the Canadian Geological Survey and is moving from Washington to Ottawa.

ALFRED H. BROOKS, geologist in charge of the Alaska division of the United States Geological Survey, has been appointed a member of the Alaskan Railroad Commission, and is now in Alaska. During his absence, Geo. C. Martin is acting chief of the Alaska division of the Survey.

MR. V. H. HUGHES, E.M., has been appointed assistant state geologist of Missouri.

THERE has just been issued a new publication by the West Virginia Geological Survey, Morgantown, W. Va. This volume is described in the following extract from the printed circular of the Geological Survey:

(1) Detailed County Report on Doddridge and Harrison Counties, under date of September 1st, 1912, 712 pages + xvi, with 29 plates of illustrations and 5 figures in the text, and a case of 3 maps (soil, geologic and topographic) of the entire area in single sheets. In addition to the detailed study and description of all the rocks, coals, limestones, clays, minerals, soils, streams and industries, with hundreds of oil and gas well records, occurring within the area and given in this Report, the geologic map gives the structural contours on the Pittsburgh coal, and thus is very valuable to any one interested in coal, oil, or gas, in showing the exact positions of all the anticlines, synclines, and structural terraces. The line where the Pittsburgh coal of commercial thickness and value disappears in Doddridge county is shown

with much more accuracy on these maps with their larger scale of one inch to the mile, and containing all roads, streams, houses, etc., than was possible on the State map with its much smaller scale. The soil map and report by the experts of the U. S. Department of Agriculture, and the topographic map, exhibit the usual features given in other County Reports. Price, with case of maps, delivery charges paid by the Survey, $2.00, but in combination with other publications of the Survey, see general circular of same. Extra copies of geologic, or topographic map, 50 cents each.

DR. REGINALD A. DALY has been appointed to the head of the geological department of Harvard University, filling the Sturgis-Hooper professorship and succeeding Professor William M. Davis. Dr. Daly was geologist for the Canadian government from 1901 until 1907. He then became professor of physical geology at the Massachusetts Institute of Technology.